# THE TECHNIQUE OF EXPERIMENTATION ON THE PSYCHO-GALVANIC REFLEX PHENOMENON AND THE PHENOMENON OF TARCHANOFF. II.[1]

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## III. THE MEASUREMENT OF THE T-PHENOMENON.

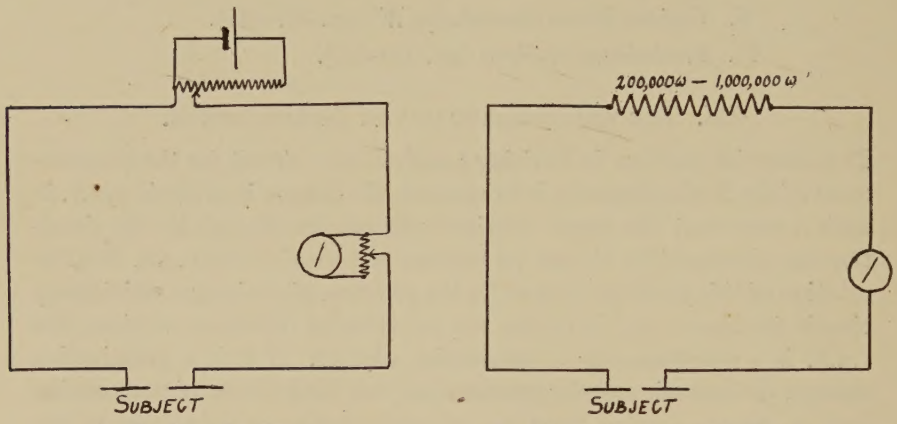
THE essential problem in devising a satisfactory circuit for the measurement of the T-phenomenon is to measure the change in somatic E.M.F. in such a way that the result obtained will not be affected by the simultaneous polarizability change (or resistance change) of the P.G.R. For the solution of this problem (just as for the problem of devising a satisfactory circuit for the P.G.R.) it makes not an atom of difference whether the P.G.R. is a resistance or a polarization change. If it is a polarization change, the somatic current produces its own back E.M.F. of polarization just as does a current from an external source, so a decrease of the subject's polarizability causes an increase in the somatic current indistinguishable from that caused by an increase of the somatic E.M.F. How is the true increase of somatic E.M.F. to be separated from this effect?

There are two methods of reducing the effect of the P.G.R. to a minimum while still recording the T-phenomenon. Tarchanoff's method (7) is by the use of the circuit shown in Fig. 5 *A*. The current change in the P.G.R. is proportional to the total current through the body. If the total current is made small by neutralization of the somatic E.M.F. ($\epsilon$) by means of an external E.M.F. ($E$), the P.G.R. effect will be reduced while the T-phenomenon will remain unchanged. An external E.M.F. is introduced into the circuit by means of a potentiometer in an opposite direction to the somatic E.M.F. until the resultant current through the body is zero (so $E = -\epsilon$). Fig. 2 *A* (2) shows a T-phenomenon obtained by the method of Tarchanoff. The P.G.R. is not entirely

[1] The first part of this paper appeared in this *Journal*, Vol. XX, 3, 1930.

eliminated by this method, since there is a deflection at the end of the reaction and this deflection has been increased by the P.G.R. by a fraction of its total amount equal to $\Delta W'/W'$—*i.e.* with an average P.G.R. the error will be of the order of 10 per cent. The error would be zero only if the deflection were zero at the end of the reaction, a condition impossible to secure in practice. This method has the advantage that the current changes are not too small for convenient measurement with a reasonably sensitive galvanometer, but has the disadvantage of requiring repeated adjustments of the potentiometer.

An alternative method which I have generally used is shown in Fig. 5 *B*. No external current is used but a large resistance is placed in




A. Tarchanoff's circuit. B. Alternative circuit for T-phenomenon.

Fig. 5.

series with the subject. This resistance must be made so large that the change in $W'$ in the P.G.R. is negligible in comparison with it. The galvanometer must be sensitive enough to give a reasonable deflection for the very small current which will flow through such a circuit. Its change of deflection will then be a measure of $\Delta\epsilon$ with negligible influence of $\Delta W'$. This method has the advantage of eliminating the need for any adjustments during the course of the experiment. It has, however, several disadvantages which are the result of the extreme smallness of the currents used (the top record in Fig. 2 *C* records a current of $1{\cdot}2 \times 10^{-8}$ amps., while the current change is only $1{\cdot}5 \times 10^{-9}$ amps.). First, even sensitive galvanometers give only small deflections for currents of this order, so high precision of measurement is impossible. Secondly, scrupulous care must be taken in the insulation of the whole circuit since

stray currents of this order are possible. Thirdly, the external resistance used is of the same order as the resistance of the insulation in some electrical apparatus (for example, on switches with a slate base), so bad mistakes in the calculation of results may be caused by the presence of such insulation in the circuit.

We may calculate the order of error produced in these different methods by using the values of the P.G.R. and T-phenomenon shown in Fig. 2. If we have a circuit with external E.M.F. $= E$ and external resistance $= R$, then $i_1$ (current before the reaction) $= \dfrac{E + \epsilon - p}{W + R}$ and $i_2$ (current after the reaction) $= \dfrac{E + \epsilon + \Delta\epsilon - (p - \Delta p)}{W + R}$. Since, for small values of $i$, $p$ will be proportional to the current, we can without error throw these equations into a form more convenient for our present purpose by treating the changes in $p$ as changes in the subject's "apparent resistance." Then

$$i_1 = \frac{E + \epsilon}{W' + R} \text{ and } i_2 = \frac{E + \epsilon + \Delta\epsilon}{R + W' - \Delta W'},$$

and

$$i_2 - i_1 = \frac{(R + W')\,\Delta\epsilon + \Delta W'\,(E + \epsilon)}{(R + W')\,(R + W' - \Delta W')}.$$

The condition for the satisfactory measurement of the T-phenomenon is that $i_2 - i_1$ should be determined mainly by $\Delta\epsilon$ and only to a negligible extent by $\Delta W'$. In Tarchanoff's method $R = 0$ and $E = -\epsilon$, so $i_2 - i_1$ reduces to the form $\Delta\epsilon/(W' - \Delta W')$, in which $W'$ has disappeared from the numerator but remains in the denominator[1], causing an increase of the T-phenomenon deflection by a fraction of its total amount,

$$\Delta W'/(W' - \Delta W'),$$

or approximately by $\Delta W'/W'$. In the curves shown in Fig. 2, $\Delta W'/W'$ was about 0·115. Fig. 2 *A* (2), therefore, which shows a change in deflection of 5·1 mm., must (if we ignore the slight additional error due to the fact that the initial deflection was not quite zero) be regarded as made up of about 4·5 mm. T-phenomenon and 0·6 mm. P.G.R. An error of over 10 per cent. is thus made in the measurement of the T-phenomenon by this method, due to incomplete elimination of the P.G.R. To this objection must be added some measure of uncertainty in the expression of $\Delta\epsilon$ as a voltage change, since its value as a voltage change depends on the value of $W'$ which cannot be measured with great precision by this method.

[1] $W'$ would only disappear altogether from the formula if $E$ were made equal to $\epsilon + \Delta\epsilon$ (*i.e.* on the condition already mentioned, that the deflection was zero after the reaction).

If $R$ were made sufficiently large for $\Delta W'$ to be negligible, the expression for $i_2 - i_1$ would be reduced to $\Delta\epsilon/(R + W')$. Again we can discover from the curves in Fig. 2 how large $R$ should have been for this condition to be approximately fulfilled. If $E$ is zero (method of Fig. 5 $B$), $i_2 - i_1$ will be $\frac{(R + W')\,\Delta\epsilon + \Delta W'.\epsilon}{(R + W')\,(R + W' - \Delta W')}$. In this formula the influence of $W'$ will be much larger in the numerator than in the denominator, so, as a close approximation, we may calculate the conditions under which $\Delta W'.\epsilon$ will be negligible in comparison with $(R + W')\,\Delta\epsilon$. The percentage error in neglecting $\Delta W'.\epsilon$ will be $\frac{100.\Delta W'.\epsilon}{(R + W')\,\Delta\epsilon}$. In Fig. 2 $C$, $\epsilon$ was found to be 0·026 volt, while $\Delta\epsilon$ was 0·003 volt. Taking $\Delta W'$ as $0{\cdot}115 \times W'$, this makes the percentage error equal to $\frac{100 \times 0{\cdot}115 \times 8{\cdot}7 \times W'}{R + W'}$ or very nearly $\frac{100 \times W'}{R + W'}$. Thus the error by this method is the same as that by the method of Tarchanoff if $R$ is only $8\,W'$. If $R$ is made greater, the error is less. The current becomes inconveniently small if $R$ is made much greater than 200,000 ohms (the value it had for the recording of the curves in Fig. 2 $C$). This for $W' = 17{,}000$ ohms gives an error of 7 per cent. For many practical purposes this may be near enough, but it is not very satisfactory. A safe rule for this method would be that the external resistance must be not less than 20 times $W'$ (giving an error of about 5 per cent.).

A much closer approximation to the pure T-phenomenon can, however, be got by combining the two methods. If $E$ were made equal to $\epsilon$, and an external resistance were used, the percentage error due to the P.G.R. would be $100\Delta W'/(R + W')$. In the examples taken this would be reduced to 2 per cent. if $R$ were 90,000 ohms. For a general safe value of $R$, we may make the rule that if an external E.M.F. is used to neutralize the somatic E.M.F., there should also be an external resistance in the circuit of not less than ten times the subject's apparent resistance. This will reduce the error in measurement of the T-phenomenon through influence of the P.G.R. to 2 per cent. with a P.G.R. of $\Delta W'/W' = 11$ per cent. If large electrodes are used (of 20 cm.$^2$ or over) $W'$ will be small enough for such an $R$ to leave the currents not too much reduced for accurate measurement with a sensitive galvanometer.

The one circuit for the measurement of the T-phenomenon which is quite indefensible for scientific purposes although it has been commonly used is one in which neither an external resistance nor a neutralizing

external E.M.F. is employed, but the somatic current is passed directly through a galvanometer[1]. This simply gives a combined record of the P.G.R. and T-phenomenon with no possibility of separating the one from the other.

Some investigators have made an even more objectionable modification of this method by using electrodes of different materials (*e.g.* carbon and zinc). This is, in effect, to introduce into the circuit a very inefficient zinc-carbon cell producing an E.M.F. of small but unknown amount which will certainly change during the course of the experiment. If a small external E.M.F. is required it can be obtained in known and constant amount from an external cell with a potential divider. In no case should electrodes be used which are themselves possible sources of current.

## IV. APPARATUS.

### (i) *Galvanometers.*

For rough diagnostic experiments in electrical responses, any galvanometer can be used which gives sufficiently large deflections for the purpose in hand. Many of the unsolved problems in this subject will no doubt be solved by skilled observers using cheap long-period galvanometers. For many such purposes, no advantage would follow from the use of a more delicate and expensive instrument. There is, however, a large class of problems for which the ordinary mirror galvanometer is useless. Its period is commonly of the order of 7 to 8 seconds, a time during which no less than three changes in current take place in the complex form of the T-phenomenon. For problems involving the measurement of latent periods, the determination of the form of the photographed curves of deflection, or even the absolute height of deflection changes, a galvanometer of much smaller period is essential. The only alternative is to obtain photographic records and to correct observed deflections for the inertia of the galvanometer by the use of formulae (1), a practicable method if few observations are made but taking up far too much time if applied to the many observations necessary in any psychological investigation.

The requirements for a satisfactory galvanometer for both kinds of electrical measurement are:

[1] Indefensible for scientific purposes, this circuit has many advantages for the popular demonstration of electric responses to a non-scientific audience. The deflections are large and (unlike the Wheatstone bridge circuit) this arrangement does not necessitate continual re-adjustments to keep the spot of light on the scale. The use of the circuit should, however, be strictly confined to such laboratory Punch and Judy shows.

(1) High sensitivity (for accurate measurement of the T-phenomenon).

(2) Short period (for the purposes mentioned above).

(3) A dead-beat deflection.

(4) Insensitivity to outside vibration (for photographic recording).

(5) A reasonable tolerance of accidental large currents (since the instrument will sometimes be handled by students who are inexpert in physical measurement).

The first two of these requirements are very much the most important. The last two are non-essential respects in which different makes of galvanometer differ considerably. The third requirement can be secured in any galvanometer (not wound ballistically) by choice of a suitable shorting resistance additional to the universal shunt used for adjusting the current through the galvanometer (or, in an instrument with an electromagnet, by controlling the strength of the magnetic field). The total resistance required to make any particular galvanometer dead-beat is generally stated by the makers. Otherwise, it can easily be discovered empirically. If the shorting resistance is too large, the galvanometer will swing past its point of deflection; if it is too small, the galvanometer will be too sluggish in reaching its point of balance.

The essential difficulty in the selection of a suitable galvanometer for all purposes is that the requirements (1) and (2) are antagonistic. I know of no galvanometer with negligible period which is sensitive enough to give satisfactory readings of the T-phenomenon. Many different kinds of galvanometer can be used, all with some disadvantages.

(*a*) *The ordinary cheap mirror galvanometer* (*d'Arsonval, Ayrton-Mather, and other patterns*). The kind of sensitivity required for the measurement of the T-phenomenon in a satisfactory circuit may be taken as 200 mm. per $\mu$ amp. at 1 metre. This is a minimum requirement; a higher sensitivity is better. Any instrument maker's catalogue shows galvanometers at from £3 to £4, of sensitivity greater than this. The periods of these instruments are, however, too long, ranging from 5 to 8 sec.

If, on the other hand, we are content with a rapid galvanometer of relatively low sensitivity (which is all that is required for the P.G.R.) there are two choices.

(*b*) *The string galvanometer.* This has been largely used for such purposes as the accurate determination of latent periods. The glass string commercially obtainable has a period of only 1/200 sec., while Prof. Einthoven himself worked with one with a period as short as 1/1000 sec. As an instrument for measuring deflections it is less suitable. Its

deflections are not proportional to the current flowing through and bear no simple relation to it. This necessitates detailed calibration over the whole range of deflections to be used. It can be made sufficiently sensitive to give good deflections for the T-phenomenon only when this is in a low resistance circuit. For this purpose the string must be made very slack. Its sensitivity then shows continual large change all the time it is being used, so detailed calibration must be repeated for every separate observation.

The expense of the string galvanometer makes it impossible of acquirement by most psychological laboratories. There is, however, a cheaper instrument which is just as satisfactory that should find a place in every psychological laboratory in which accurate research on the time relationships of these phenomena is required.

(*c*) *The Kipp galvanometer.* This is an instrument with a very small mirror and a single loop of wire in a magnetic field. Its period is 1/50 sec. Although longer than that of the glass string, this is no disadvantage for any purpose for which it could be used in psychological investigation. It is convenient to use since its deflections (if small) are proportional to the current, and it works in any position. Its sensitivity is not great enough for T-phenomenon readings except in the unsatisfactory low resistance circuits. A further disadvantage is the ease with which it can be damaged by excessive currents.

(*d*) *The Moll galvanometer* is probably the best single compromise between the requirements of short period and high sensitivity[1]. Its sensitivity is just great enough for taking satisfactory readings of the T-phenomenon in high resistance circuits, while its period is about 1·4 sec. This period is short enough for most practical purposes, although not for very accurate determination of latent periods and maximum and minimum points. The Moll is very tolerant of excessive currents, but has the disadvantage of responding too easily to external vibration.

(ii) *Electrodes.*

Satisfactory electrodes should fulfil the following conditions:

(1) They should not themselves be possible sources of current. This is secured by making them of the same material. A further refinement, if they are of metal, is to cut both from the same strip. Soldered junctions must not be within reach of the electrolyte and are best kept quite away from the surface of contact.

[1] The photographic records shown in Fig. 2 were made with a Moll galvanometer for which I am indebted to a grant made by the Industrial Fatigue Research Board.

(2) They should not produce a back E.M.F. of polarization when a small continuous direct current is passed through them. Such a back E.M.F. will be produced if the passage of current causes either a change in the chemical nature of the surface in contact with the skin or in the concentration of the electrolyte. Plates of zinc (as of any other metal) whether dry or moistened with a solution of common salt would be objectionable for the first of the above reasons; plates of zinc in bags moistened with zinc chloride solution would be objectionable for the second reason.

(3) They should form sufficiently good contact with the skin for this contact to be undisturbed by movement.

(4) At the same time, they must be comfortable and not fastened so tightly as to restrict the blood supply.

(5) There should be no possibility of progressive drying of the electrolyte or similar change.

These points are of very unequal importance. No. 3 is clearly most important for our purpose since this alone is a way in which unsatisfactory electrodes might lead to current changes simulating the reaction studied. Polarizability in the electrodes, on the other hand, could at worst lead to partial obscuring of the phenomenon and an artificial increase in the value of $W'$. These are to be avoided as far as possible, but scrupulosity about the non-polarizability of electrodes does not compensate for errors in other parts of experimental technique.

It is generally stated that no electrode is perfectly non-polarizable. Liquid electrodes can probably be made most nearly so. A simple non-polarizable liquid electrode is described in a recent number of *The American Journal of Psychology* (12).

The electrodes which I have used are similar to a pattern described by Dresbach (13). From the same strip of thin silver foil (of about 0·05 mm. thickness) two pieces are cut out. Each consists of a circular area of about 20 cm.$^2$, with an extension in the form of a straight strip about 1 cm. wide and 4 cm. long. Copper wires are soldered or otherwise attached to the ends of these strips. The electrodes are then coated with silver chloride by electrolysis in a sodium chloride solution. For attachment to the subject one surface of the electrode is coated with a thin film of gelatine moistened with Ringer solution. As an additional precaution against polarization, I use for this coating an emulsion of silver chloride in gelatine, but I do not know whether this is necessary or useful. The large size of the electrodes keeps the total resistance low and prevents evaporation. The electrodes are bound on with a bandage liberally padded

with cotton wool; a woollen glove can be used in addition or can replace the bandage if there is sufficient padding. The guarantee of efficient contact is that on removing the bandages at the end of an experiment, the electrodes will be found stuck to the skin over their whole area, and, when they are peeled off, they are found to carry over their whole surface a sharp impression of the lines, hairs, etc., on the skin.

### V. VARIOUS FACTORS DETERMINING $W'$.

It is well known to all experimenters that on first attaching electrodes to a subject he generally shows at first a very high resistance, differing largely from day to day, which drops during a period of from twenty minutes to half an hour. This change is probably the resultant of many different factors, its speed depends on the nature of the electrolyte and we may safely guess that penetration of the high resistance layer of the skin by the electrolyte is the predominant factor. This must, however, be complicated by other physical changes taking place at the electrodes (the rise of temperature at the skin surface, for example, which we know is not the predominant cause of the phenomenon since the initial fall of resistance is also found when the electrodes are attached to a corpse).

The interest of the absolute value of $W'$ to the psychologist is that it is (other things being equal) an indication of the mental condition of the subject. I have expressed this mental condition as 'alertness' or the readiness to react to a stimulus(10); Mr Cattell considers that $W'$ measures the fraction:

$$\frac{\text{Available instinctive energy}}{\text{Extent of release of energy taking place in consciousness}}$$ (14).

The difficulty in making any use of measures of $W'$ is the number of non-psychological factors which also influence its value, some of which only are controllable. The attempt to understand these and to control them as far as possible is necessary also in measurements of $\Delta W'$, since $W'$ is the background against which P.G.R. changes occur and we are by no means certain that we can make significant comparisons of P.G.R. changes which occur with different values of $W'$.

(1) The first of these factors is the whole set of physical changes which determine the physical part of the initial fall of resistance. These changes cannot be avoided but can best be dealt with by putting off measurements until at least twenty minutes (better until half an hour) after the electrodes have been attached. Early investigators often measured the P.G.R. against a background of falling apparent resistance soon after electrodes were attached. There is nothing to be said for this practice and

it is now generally given up. Concentration of electrolyte will of course be carefully controlled, and to prevent osmosis from bodily cells it seems better to use a solution isotonic with the body fluids rather than 5 per cent. or 10 per cent. salt solutions which would tend to dehydrate the skin cells thus possibly altering their resistance.

(2) If half an hour after electrodes have been attached a subject walks about, his resistance will be found when he sits down again to be lowered and to be rising. This effect of general movement is another reason for making no observations for some time after the experiment has started. It will be noticed that the effect of this factor is in the opposite direction to that of the last one mentioned. It is possible that the discrepant results obtained in different observations of the initial change in resistance (which sometimes is a rise instead of a fall) are due to the occasional preponderance of this factor over the last, since at the beginning of an experiment the subject has probably been moving about.

(3) When a current starts flowing through the skin there is at the moment of contact no polarization, so the apparent resistance rapidly increases from a low to a high value as the polarization reaches its full value. This we may call the 'primary polarization change.' Experiment with string galvanometers shows that the greater part of the polarization is attained in a fraction of a second and that it is virtually complete in a few seconds. It is, therefore, too quick to be any trouble in practice. We are unlikely to try to measure $W'$ in less than four or five seconds after completion of the circuit.

(4) There is, however, a slower change (which we may call the 'secondary polarization change') which sometimes requires consideration. The passage of a continuous current causes a decrease of polarizability[1]. Thus when a current begins to flow in the body, or when a continuous current is considerably increased, there is first a large current, decreasing for a second or two as a result of the primary polarization change, then the decrease changes into a slow increase as a result of the secondary polarization change. This change is slower. Three minutes is probably a safe time to allow for equilibrium after a considerable change in current has been made, but one minute is ample for most practical purposes. The P.G.R. should not normally be measured in much less than a minute after a considerable current change.

(5) Part of the secondary polarization effect is temporary, the change being reversed after the current has ceased. Part, however, is permanent,

[1] Einthoven considers that this is a change in true resistance caused by the passage of a current. I have evidence against this view, but in practice the difference does not matter.

and this part calls for an additional control. If a very large current is passed through the skin, its apparent resistance to a small current is found to be very greatly reduced and to remain at a lower level. For example, after electrodes had been attached for half an hour, I measured the apparent resistance of a subject with an E.M.F. of 2·1 volts and found it to be 43,800 ohms. An E.M.F. of 22·8 volts was then applied for one minute, at the end of which $W'$ as measured by that voltage was 2280 ohms. It was then measured again at the lower voltage and was found to be 3805 ohms and remained at that sort of level. This observation indicates that, in order to get significant values of $W'$, it is necessary to control not only the E.M.F. at the time of observation but also previous E.M.F.'s applied.

(6) The secondary polarization effect has a curious influence on $W'$ when measured by electrodes on the back and palm of the hand. Change of the direction of the current causes either an increase or decrease of $W'$, an increase if the anode is changed from the back to the palm and *vice versa*. This is apparently due to the secondary polarization differing in amount at the anode and kathode, and also differing in its effect on the skin of the palm and back of the hand. Veraguth states that the apparent resistance of the palm actually increases when current passes through it, while that of other parts of the skin (except the sole of the foot) decreases. If this is so, the effect of reversal on the hand resistance is explained if we assume that the secondary polarization change is more effective at the kathode than at the anode. Whatever may be its explanation, this phenomenon also points to the necessity for controlling the direction of the current. This necessity probably does not apply only to the hands, but in a lesser degree to all other parts of the skin.

(7) There remain a large number of factors influencing the size of $W'$ which are not known and cannot therefore be controlled. Even after washing the skin with alcohol the apparent resistance of a subject varies from day to day with identical electrodes and under identical experimental conditions, and even from minute to minute in the course of a single experiment. There is no evidence that these differences are altogether indicative of general changes in the condition of the subject of any psychological interest. There is ample evidence that they are so in part, but they may also be in part due to differences in the skin condition of purely local significance. Particularly is this probable in the variations of $W'$ from day to day. The usefulness in psychology of electrical measurements and particularly of measurements of $W'$ would be much enhanced if we knew any method of entirely eliminating these variations.

## VI. Preliminary problems.

The usefulness of the P.G.R. and the T-phenomenon as instruments of psychological measurement depends largely on the solution of some preliminary problems. We should like to be able to use the shape of the electrical response curve as an objective indication of the character of response of a subject to stimulus, so that unscientific descriptions in terms of popular emotional and conational terms (such as "he felt afraid" or "he decided to do so and so") may be replaced by objectively verifiable indications of the nature of the physiological pattern of response. We should like also to be able to give a quantitative measure of such responses.

It is clear that we need as full an understanding as possible of the physiological response before we can get very far in correlating forms of the curves of electrical response with different conational and emotional responses. Much work on the physiological side is being done, such as Densham and Wells's demonstration that changes in apparent resistance may follow from the constriction or extension of the skin with vasomotor changes(15). Yet it is difficult to believe that the physiological problems of the phenomenon are solved. The complex form of the T-phenomenon suggests the presence of different physiological components with different time relationships. If there are specific differences in the P.G.R. response to different situations, it seems too that these must be the result of differences in the action of different components in the physiological reaction. The most hopeful first step in the study of such specific differences would seem to be the separation of these different components.

Many investigators prefer to make a bold attack on this more complex problem without waiting for the success of any preliminary investigation. There is much to be said for this attitude. It must be remembered, however, that such a problem as that of specific differences in the forms of curves can only be attacked with the finest instruments. Curves for this purpose obtained with long period galvanometers or on unsatisfactory circuits are of no scientific value whatever.

The use of the electrical responses as indicators of the quantity of response also necessitates preliminary research into more simple problems. The simplest of all is a demonstration of the quantitative relationship between responses to controllable stimuli of different physical intensities. This is an important and very simple matter which has received surprisingly little attention. Then there is the question of whether

$\Delta W'/W'$ is a significant measure of the extent of the P.G.R. for different values of $W'$[1]. The settling of this question is absolutely essential to any comparison of sizes of P.G.R.'s except under the very unusual conditions that the P.G.R.'s compared have been measured at the same value of $W'$.

The problem of the physical nature of the P.G.R. phenomenon (whether a resistance or polarization change) has generally been regarded by psychological investigators as no concern of theirs. With some exceptions, the psychological investigator is concerned with problems in which this difference is unimportant. One of the exceptions is, as indicated earlier, that many of the factors in experimentation (such as the voltage and constancy of $e$) which would not need control if we were measuring a pure resistance change, need in fact careful control if we are dealing with a phenomenon of polarization. The other point at which the difference becomes important is the question of the significant measure of the P.G.R. The theoretically probable significant measure depends on the physical nature of the change. The physical nature of the change is, therefore, also one of the preliminary problems of the P.G.R. About the physical nature of the T-phenomenon there appears to be no dispute.

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[1] Mr R. B. Cattell has realized the importance of this problem and has published figures tending to show that $\Delta W'/W'$ is less for high values of $W'$(16). His circuit was not ideal for this purpose, and it is to be hoped that these figures will be confirmed on a circuit with adequate elimination of the T-phenomenon and with P.G.R.'s obtained from a physical stimulation of exactly controllable intensity.

(*Manuscript received* 14 *May*, 1929.)

# THE TRANSFER OF TRAINING[1].

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At present a somewhat paradoxical situation exists with regard to the problem of transfer. In the majority of experiments transfer has shown itself either not at all or only in a degree that may be described as statistically and theoretically significant but practically unimportant. Yet many psychologists are convinced of the practical importance of transfer. It would hardly be reasonable to accept without reserve an opinion not consistent with the bulk of the available experimental evidence, but the prevalence and strength of such a belief do seem to call for some sort of stocktaking. It may be asked whether experiment has clinchingly exhausted all the possibilities and whether the technique used has been completely satisfactory. The answer must, I think, be not only that these two requirements have not been met even in a limited practical sense, but also that some of the methods used have been such as to diminish the likelihood of finding any transfer at all.

Transfer investigation originated from the desire to test the doctrine of Formal or Faculty Discipline. A technique suitable for this purpose was developed and an experimental decision reached. After the rejection of the view that there are general mental powers which can be improved by practice in one field and which can carry over this improvement to other fields, there came attempts to re-state the belief in transfer in a form that implied no belief in Faculty Training. Experiments were set up to test these revised beliefs, but these experiments were framed on the general plan of the earlier work. This was unfortunate because the Faculty Training Technique is unsuitable for the solution of modern transfer problems. In what this unsuitability consists will appear presently.

If transfer takes place at all and if it does not take place through the medium of general mental powers, then it is natural to consider it as occurring between mental functions, that is, between such systems of habits, such products of learning as skills or abilities. (One speaks of the

[1] This paper was read before Section J at the South African Meeting of the British Association, July, 1929.

ability to typewrite or the skill of typewriting or of the mental function of typewriting, the different terms emphasizing different aspects of substantially the same entity.)

Each such ability is no doubt based upon innate factors; but as it expresses itself in actual behaviour and experience, it is essentially a learnt system, an acquired ability. Each such ability may further be thought of as consisting of a set of constituent abilities or functions, integrated and organized in a particular way. Thus arithmetical ability may be thought of as consisting of the ability to add, the ability to multiply, and the like; and these constituent abilities can conceivably be analysed into the ability to multiply 2 by 3, and so on. It is not of course implied that such constituent abilities are identical when functioning as constituents of different abilities, that is, in different settings. Part of the problem is to discover how far there can be identity in such cases. Nor is it implied that the analysis of an ability into its constituents can be satisfactorily carried out in any but an experimental way. The analysis of arithmetical ability sketched above is for illustrative purposes only and is not guaranteed. But pending the development of a method of analysis, it may be permitted to think of abilities in the way described in so far as the thinking leads to an experimental issue.

Abilities then are provisionally to be thought of as made up of constituent abilities, co-ordinated on a particular plan, without any implication that either the pattern of organization or the abilities organized are independent of one another. As constituents may be included such abilities as that of taking up an active problem-solving attitude, that of resisting certain distractions, that of working up to a certain level of achievement, and the like. Whether constituents differ in kind as well as in complexity and specificity, is not material here.

It is the common practice to designate abilities by the product produced, the function performed or the purpose served. As a name, as an indication of the object of thought, this is useful; as a description it is clearly inadequate. Thus one person's ability to typewrite and another person's ability to typewrite might be thought of as differing only in degree, not in kind. Both produce typescript. Yet one may use the touch method, the other the sight method; one may use both hands, the other only one. These are gross differences, but even in the absence of such obvious differences it cannot be argued that differences do not exist or that existing they are rare and unimportant. A better plan would be to describe abilities in terms of their constituents and of the mode of organization of the constituents within the ability. It would not be

possible to do this very fully as yet. But to attempt to do it would at least save one from thinking of abilities identical in purpose or product as identical in all other respects.

The training of an ability may involve changes in the constituents—constituents being omitted or modified and new ones introduced; and it may involve changes in the organization of the constituents. Both kinds of changes, changes in constituents and changes in organization, are qualitative changes. They may be distinguished collectively from that quantitative change which results from training and which consists in a facilitation change, in a change in the working of the constituents. A facilitation change does not necessarily involve any qualitative change; in fact, however, training is apt to bring about qualitative as well as quantitative changes, and it should never be assumed without proof that the effect of training is merely a facilitation change. Adequately to describe the effect of training would be to describe qualitative as well as quantitative changes.

Usually the effect of training, being thought of mainly or solely as a facilitation change, is expressed in terms of functional efficiency and measured by change in the quantity of product per unit of time, by change in liability to error, and the like. Now such measures are reasonably adequate so far as facilitation changes are concerned. The effect of a qualitative change is, however, far less likely to be accurately measured in this way. The change in efficiency may be potential rather than actual. A change from the sight to the touch method of typewriting may at first result in a diminution of efficiency and any potential superiority in respect of a higher limit of attainable efficiency may not manifest itself until much more training has been given. Generally it may be said that a qualitative change in an ability is less likely to be immediately and adequately reflected as a change in efficiency than is a facilitation change. Time is needed for the potential change in efficiency to actualize itself.

It is now possible to re-state the problem of transfer. Transfer of training refers to those changes (if any) in the facilitation and in the constituents and organization of an ability which result, not from direct training of that ability but from training some other ability. In other words, it refers to those qualitative and quantitative changes in one ability which result from qualitative and quantitative changes in another ability. The problem of the transfer of training concerns itself with the nature, magnitude, extent and direction of such changes and with the conditions which determine them.

Adequately to investigate such a problem or set of problems it is

clearly necessary to choose very carefully the abilities between which transfer is to be expected; for it is certain that transfer does not occur in appreciable amounts between any two abilities, and as transfer does seem to occur in certain cases, it would seem advisable to work outwards from the investigation of these cases rather than to cast one's net at random. Further, the abilities chosen ought to be described in regard to their qualitative as well as to their quantitative state. The training of the transferring ability should be very strictly regulated and observed, and its qualitative and quantitative development described. Finally long and closely regulated and observed training tests should be used for the purpose of detecting transfer effects, both for the control and the training groups. And search should be made for qualitative as well as for quantitative differences between the two groups.

An experimental technique satisfying these general requirements certainly does not exist at present and will take long to develop. In the meantime these requirements should be borne in mind when transfer experiments are being planned. Yet how often are the abilities chosen almost at random and defined merely in terms of product or function or purpose! How often is the training essentially unregulated and unobserved, and its effects measured only by short immediate efficiency tests! Such considerations are negligible when it is a question of the doctrine of Formal Discipline. To neglect them in transfer experiments is to imperil the whole experiment. For if one takes a group of individuals and limits them in regard to little more than kind of product and amount of time spent in learning to produce that product, it is probable that at the end of the training the group will be far from homogeneous in respect of the ability trained, whatever may have been the case at the outset. They will produce the same kind of product but they will do it in different ways, in other words there will be qualitative differences between abilities nominally only quantitatively different. Without close observation of the training and minute description in terms of constituents and pattern of organization, the nature of these differences will be unknown. Further, if brief immediate efficiency tests, which ignore qualitative changes, are the sole indicators of transfer effects, the possibility of any transfer at all being found is significantly reduced.

It has already been said that the problem of transfer concerns itself with the changes in constituents, organization and facilitation that occur in one ability as the result of changes in another ability in these respects. It will perhaps be helpful if a brief survey of the field of investigation is made from this standpoint. This survey, necessarily brief, deliberately

concerns itself with those cases where transfer has actually been found or where it may reasonably be expected. No attempt is made to discuss the practical importance of transfer.

First may be considered the case of facilitation changes in Ability A due to facilitation changes in Ability B. This coincides roughly in scope with the Theory of Transfer by Common Elements in its positive form. In its most general positive form that theory assumes that if there are constituents common to two abilities, and that if one of the two abilities is modified in regard to its state of facilitation by practice, then the other ability will be modified in virtue of the common constituents[1]. It seems probable however that this statement is not unrestrictedly true, that at least the magnitude and time of incidence of the transfer will vary with certain conditions. If for instance the increase of facilitation in Ability A refers rather to the working together of the constituents than to any improvement of the individual constituents, if the facilitation is one of the integration of the constituents, then unless not the simpler constituents but at least one organized set of simpler constituents is common to the two abilities, there seems no reason to expect transfer to occur.

Generally the problem is one of facilitating by practice one ability and ascertaining under what conditions the facilitation transfers to another ability containing some of the same constituents or at least some that are overtly the same. Many kinds of cases are possible. The constituent abilities can be trained separately before combination in one inclusive ability. Or the reverse case can be taken—the inclusive ability formed first and then the constituents taken alone. Or the case of constituents included in two different abilities. In all these cases it may be questioned how far the amount of transfer is likely to be independent of the stage of learning reached and the strength of the integration of the constituents in the abilities concerned.

The problems involved are perhaps less obscure where the one ability is in process of formation and incorporates constituents of the other more or less trained ability. It seems probable that in such cases considerable

[1] I do not here raise the question of the extent to which one can safely argue from the overt similarity of constituents to adequate functional identity of the underlying psychophysical processes. But clearly unless sufficient identity can be assumed, *i.e.* identity in so far as transfer is concerned, the basis of the theory disappears. The assertion that common elements exist, or at least that there is community of function adequate for transfer purposes, is fundamental. But it seems to me that this assumption is better tested by the sort of investigation indicated above than in any other way. From one point of view investigation in this field is an enquiry into the existence and extent of the underlying identity of overtly similar constituents.

re-adjustment will be required—the transferred constituents have to be integrated into their new setting, organized into a new pattern and with new associations, the process being accompanied by more or less interference from the old setting. It seems likely that time will be needed for any potential transfer to be actualized, for integration and re-organization to reach a level at which efficiency rises above zero or above the initial score, and for interference to abate. Both time and practice are necessary for the manifestation of the transfer. One illustration of this may be seen in Pyle's card-sorting experiments. His Group B, after 15 days' practice with one arrangement of the boxes (Scheme 1), was asked to sort the cards with a different arrangement (Scheme 2). On day 1 (Scheme 1) their average time per sorting was 750 seconds, on day 1 (Scheme 2) it was 325 seconds; and day 15 (Scheme 1) was equalled by day 4 (Scheme 2) with 150 seconds per sorting[1]. Thus the full amount of transfer did not appear till the fourth day, and although considerable transfer did appear on the first day, it must be remembered that the initial score for Scheme 2 is the average score for the whole of the first day's work. Had the score for the first sorting been given instead, it is probable that the amount of immediate transfer to be recorded would have been appreciably diminished.

Another illustration is given by one of Book's subjects (Subject X). This subject, after 85 hours' practice at typewriting by the sight method, had reached a speed of 1600 strokes per minute from an initial speed of 300 strokes per minute. After a five months' interval the subject re-started typewriting, this time by the touch method. The initial speed was less than 200 strokes a minute and a speed of 1600 strokes a minute was not reached till after 60 hours' practice. In this case it seems likely that no positive transfer would have shown itself in an immediate brief test. As it was, a saving of about 25 hours appeared[2].

Cases capable of similar interpretation are not uncommon. The ability with which we respond to some more or less novel situation is frequently a modification of some already formed ability or contains

[1] W. H. Pyle, "Transfer and interference in card-distributing," *J. of Educ. Psychol.* 1919, x, 107–110. The figures given are read off to the nearest 25 from Pyle's Fig. 1. Pyle's experiments were performed for a purpose somewhat different from that of the text and illustrate rather than establish the interpretation given here. The same remark applies to the other examples given in this paper.

[2] The facts are taken from E. L. Thorndike's *Educational Psychology*, II, 138–139, Figs. 58 and 61. That the drop to 200 strokes a minute at the beginning of the touch typewriting was not due to forgetting, is, I think, clear from the facts about X's forgetting given on p. 311, Table 31, as well as from other considerations.

constituents practised in other settings. In such cases more or less re-adjustment is necessary, more or less interference is operative, and the full amount of the transfer only appears when the re-adjustment is complete and the interference has abated. In this way can be explained the very rapid early improvement found in some cases of learning—for instance, Thorndike's finding that as a result of 7 hours' practice the ordinary college graduate will diminish the time required to do mental multiplications to two-fifths of the initial time[1]. Thorndike, after reviewing a number of cases of the learning of abilities under experimental conditions, draws attention to the frequency with which a very rapid rate of improvement occurs[2].

How much of the lag of the transfer is due to interference, and how much to re-adjustment apart from interference, is not clear at present, and for most purposes it is convenient to lump them together without any attempt at separation. But certain general considerations appear. It would seem that interference is maximal where the transferring and recipient abilities, and the situations to which they are adjustments, differ very little. When situation, pattern, and all constituents save one are identical, they will all condition a strong facilitatory tendency towards the constituent with which they have been previously integrated and a strong inhibition against the new constituent with which they are now to be integrated. The learner combats this by giving the new constituent a mental prominence which, while necessary at the outset in order to overcome the interference, is only temporary, and has to be modified later on if the most effective ability is to be developed. The more the abilities differ in situation, pattern and constituents, the less strong the interfering tendency and the greater the necessity for re-adjustment. The transferred constituents have to be integrated together with new constituents into a new pattern adapted to a modified situation.

Newkirk and Gundlach[3] have recently carried out an experiment which bears on this question. They have shown that training in the cancellation of 6's produces a significant positive transfer to the cancellation of *d*'s but not to the cancellation of 2's. A short test was used as the indicator, and so it is not possible to say decisively whether or not there

[1] Cf. E. L. Thorndike, *op. cit.* p. 148. I refer here to cases where the initial improvement is uncommonly rapid. How far the negative acceleration general in learning curves is due to the factors of transfer and interference and re-adjustment is not dealt with here. It might be worth investigation.

[2] E. L. Thorndike, *op. cit.* pp. 150 and 157.

[3] "The transfer of training in cancellation experiments," *J. of Educ. Psychol.* 1929, xx, 291 ff.

was potential transfer in the case of the 2's. As, however, in spite of the interference that existed, the training group did no worse than the control group, it seems probable that a long test would have demonstrated transfer in the case of the 2's. The amount of interference in the case of the *d*'s must have been much less, for the figure blank contained only figures and the letter blank only letters. Had the blanks contained both figures and letters, there would have been interference between the tendencies to strike out a *d* and to ignore it and between the tendencies to strike out a 6 and to ignore it, and this interference would have been all the greater owing to the similarity of the situation, pattern and other constituents. Just as there was in the case of the 6's and 2's.

So far in this paper the discussion has been confined to the set of problems relating to the extent to which, and the conditions under which, facilitation changes in one ability can produce facilitation changes in another ability, given common constituents. Another and not less important set of problems arises in regard to the question, How far and under what conditions can qualitative changes in the one ability produce qualitative changes in another ability? How far does the nature of the constituents and of their organization in the one ability determine the nature of the constituents and of their organization in another ability[1]? If such determination occurs, it should ultimately if not immediately express itself in an efficiency change. But as efficiency changes of themselves give no indication of their causation, and as the determination will manifest itself immediately as well as ultimately in qualitative changes, attention should be primarily directed to the qualitative changes in both the transferring and the recipient abilities.

In adjusting to a situation that is significantly novel to a greater or less extent, it is frequently assumed that the learner tends to apply abilities already acquired and in some sort of relation to the similarity between the new situation and old situations to which adjustments have already been made, to the degree of familiarity of the old situation and to the degree of facilitation of the ability or response adjusted to that situation. How far the ability in process of formation is permanently modified by such tentative adjustments depends upon many conditions, principally upon whether or not the old ability can be made to serve the new purpose sufficiently well.

But initially everything depends upon whether or not the new situa-

[1] I shall not discuss save incidentally either the production of qualitative changes in one ability by facilitation changes in another ability or the production of facilitation changes in one ability by qualitative changes in another ability.

tion evokes the old ability. There are many cases where the resemblance between the new situation and the old is obvious, or, to put the matter another way, where the probability is great that the new situation will evoke the same response as the old or at least a response containing some of its constituents. There are also cases where the probability is less great, where a smaller proportion of learners would make the connection. What are the conditions which determine the probability of making the connection?

It seems likely that there will be a positive correlation between amount of intelligence and the extent to which such connections are made[1]. But it also appears likely that the probability of making such connections can be increased by modifying the transferring ability. This modification will be in the direction of generalizing the ability, that is, of narrowing down the situation to which the response is made, to the more essential specific features so that a greater number of situations can evoke the response, and possibly of narrowing down the response to the more essential constituents so that it can be used with new constituents at a smaller price in re-adjustment and interference. Greater range is secured by making the situation and response less precisely adapted in detail to any one concrete situation.

This end is achieved by pointing out cases where an ability in process of formation can be applied, by teaching general rules of application, by emphasizing the essential aspects of the situation and the response, and generally by making the methodological aspect as explicit as possible. But to do this is to produce an ability qualitatively different from one produced by a less generalized training. The pattern of organization will be different and there will be differences in constituents. But, if this is the case, efforts should be made to describe these differences directly. That they will ultimately express themselves in efficiency changes is insufficient warrant for observing only efficiency changes, particularly if short, immediate tests are used. To do this is to jeopardize one's chance of getting any efficiency changes at all and to forfeit one's chance of explaining the absence or presence of efficiency changes.

It is in the direction of transfer by generalization that the believers in transfer have looked for the most important results. And the few experiments which have been carried out in this field seem to support their expectations. The experimental evidence would be much stronger if the precautions and principles advocated in this paper had been at all

[1] The relation between transfer and intelligence still awaits adequate investigation.

generally observed. For to demonstrate transfer, when the dice are loaded against success, is of good augury for the future.

To illustrate: Ruediger inculcated the practice of neatness in connection with a certain school subject and in addition generalized the training by teaching the ideal of neatness. He obtained indications of transfer, whilst Squire, who refrained from any such generalization or explicitness, obtained none[1].

Woodrow trained two groups in the memorization of poetry and nonsense syllables. To the one group was given instruction in the best methods of memorization, practice in applying such methods to the memorization of poetry and nonsense syllables, and emphasis upon the desirability of applying such methods to other kinds of material. The group so instructed showed a clear gain over the control group in short immediate tests, whilst the group which had been left to its own devices during the period of training showed no such clear advantage[2].

In Judd's well-known experiment two groups, one of which had received instruction in the principles of the refraction of light, were trained in throwing darts at a target below water. No difference was observed between the two groups in acquiring this ability, but when the depth of the water was altered the instructed group quickly outstripped the other[3].

These three cases (taking Ruediger's and Squire's investigations as complementary) enforce the necessity for qualitative observation. The abilities given the generalized training were in all probability qualitatively different from the abilities not given the generalized training. Certainly they reacted differently to transfer opportunities. It is reasonable to suppose that close observation directed to discovering qualitative differences between the groups would not have been completely fruitless. Qualitative as well as quantitative differences should be looked for between the groups receiving different kinds of training during as well as at the end of their training, and between the training and control groups during as well as at the end of the training of the test abilities.

[1] E. L. Thorndike, *Educational Psychology*, II, 411–412.

[2] "The effect of the type of training upon transference." *J. of Educ. Psychol.* 1927, XVIII, 159–172.

[3] E. L. Thorndike, *op. cit.* pp. 400–401.

SUMMARY.

1. The problem of the transfer of training should be regarded as that of investigating the conditions under which qualitative and quantitative changes in one ability due to training determine qualitative and quantitative changes in other abilities.

By qualitative changes are meant changes in the nature of the constituents and of the organization of an ability. By quantitative changes are meant facilitatory changes in the ability. Both kinds of changes may manifest themselves as efficiency changes.

2. The abilities investigated should be carefully chosen, on the ground that transfer does not occur anywhere and anyhow but only when certain not yet fully known conditions are satisfied.

3. Transfer should be sought for in long training tests, not in short immediate tests. This is necessary in so far as efficiency is concerned, because some of the transfer may not immediately manifest itself in efficiency changes and because efficiency gains are liable to be masked at first by temporary efficiency losses due to re-adjustment and to interference. But it is also necessary in order to ascertain the qualitative transfer changes and to determine the causes of the efficiency changes.

4. The training of the transferring and recipient abilities should be closely observed and regulated, and attempts should be made to determine qualitative as well as quantitative ability differences between groups given different kinds of training as well as between training and control groups.

(*Manuscript received* 18 *October* 1929.)

# A NOTE ON OVER-TRAINING.

By EDNA M. HENSHAW and PORTIA G. HOLMAN.

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The following results on the effect of length of practice period arose incidentally from experimental work on the acquisition of manual dexterity. Three groups of 30 subjects were employed on chain assembling[1] for a training period of 80 minutes each morning. In the afternoon Group I had a further 80 minutes' chain assembling. Group II had 80 minutes' cartridge filling[2] and Group III was not employed.

Thus the morning occupations of the groups were identical, and the afternoon employment is the one variable.

The three groups were employed for a fortnight. The comparison between their practice curves in the morning chain assembling is shown in Fig. 1.

It will be seen that their performances were almost identical in spite of the fact that Group I had actually twice as much training as Groups II and III.

After a period of some months, five subjects from Groups II and III were re-employed for a fortnight, in which they continued training in chain assembling for 80 minutes each morning[3]. After a drop due to the interruption of practice, this group continued to improve at the same rate. This shows that maximum improvement had not been reached earlier, and that the failure of the concentrated practice of Group I to effect improvement was not due to any flattening of the practice curves. When these five subjects had had the same total amount of training as Group I, their output was very much higher.

These results show that increase of the daily training period beyond a certain length has no apparent effect. The extra 80 minutes' training in the afternoon resulted in no additional improvement whatever.

[1] For a description of the actual operation of chain assembling see Langdon and Yates, this *Journal*, April, 1928, xviii, Part 4.

[2] Cartridge filling was specially designed to involve movements as like those of chain assembling as possible though using different material.

[3] This interruption was due to an unavoidable necessity for the main purposes of the experiment from which the results here reported were obtained.

It is worth noting that each of the 80-minute training periods was divided up into eight 10-minute periods separated by 10-minute rest pauses. Moreover, the two 80-minute training periods of Group I were separated by a luncheon interval of an hour and a half. No appearance of fatigue was shown either in the mornings or the afternoons. The fact that Group I maintained as high an output in the afternoons as in the mornings and that Group II improved steadily in cartridge filling shows clearly that fatigue was not operative.

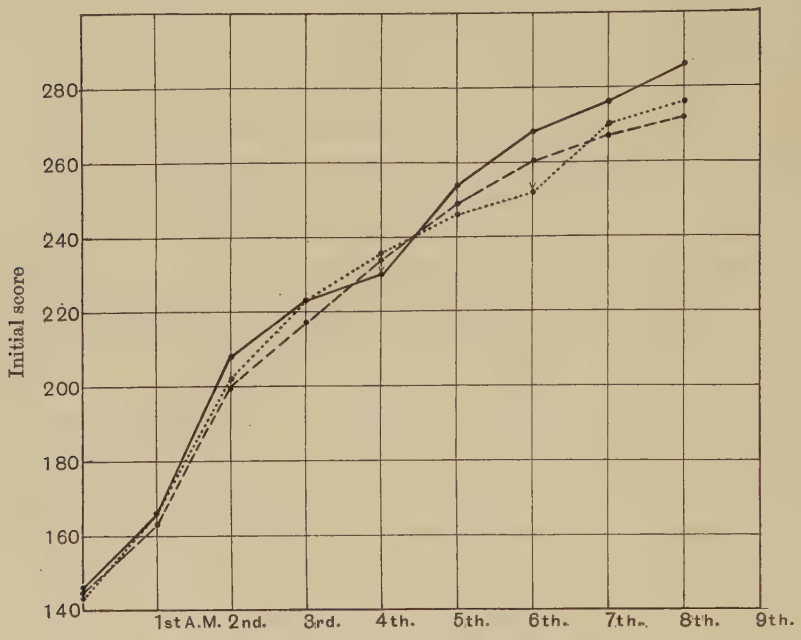


Fig. 1. Practice curves of chain assembly groups.

——— = Group I. Chain assembling A.M. and P.M. Sixteen 10-minute periods per day chain assembling.

• • • • • • = Group II. Chain assembling A.M. Cartridge filling P.M. Eight 10-minute periods per day chain assembling; eight 10-minute periods per day cartridge filling.

- - - - - - = Group III. Chain assembling A.M. Rest P.M. Eight 10-minute periods per day chain assembling.

These results further emphasize the importance of determining the optimum arrangement of training for any particular operation. It is evident that 160 minutes' training per day is well above the optimum for chain assembling, and probably for other similar operations. In fact, the 80 minutes' morning training is probably excessive.

In this comparison, *length* of daily training period has been the variable under consideration. How far the relative rate of improvement is affected by *distribution* of training is not shown in these results, but the relation between length and distribution of training period is a problem with important practical consequences, and one which could be solved by further experimentation along the same lines.

(*Manuscript received* 20 *December*, 1929.)

# AN EXPERIMENTAL INVESTIGATION INTO THE SIMULTANEOUS CONSTITUENTS IN AN ACT OF SKILL.

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## I. Statement of problem.

The relation of 'parts' and 'wholes' first gained psychological, as distinct from philosophical, significance, in the work of Lottie Steffens on the economy of verbal learning[2]. The problem was shifted from the verbal to the muscular sphere by the researches of Pechstein[3] and Gopalaswami[4] upon part and whole methods in maze-running and mirror-drawing respectively.

[1] My thanks are due to Professors Pear and Spearman for help and guidance in the planning of these experiments. This paper, moreover, was written in New Zealand, and its passage into print would have been impossible but for the further kindly assistance of Prof. Pear.

[2] *Zeitschrift für Psychologie der Sinnesorgane*, 1900, p. 321.

[3] *Psychological Monographs*, 1917, xxiii, No. 2.

[4] This *Journal*, xv.

But there is one great difference between the 'part-whole' problem of verbal learning and that of muscular skill, which has never been touched upon: the former is one-dimensional, the latter two-dimensional. A mental process is a unitary stream which can be divided only into *successive* constituents. A muscular process, on the other hand, can be split into simultaneous as well as successive constituents.

Suppose, for example, that the series of muscle, joint and tendon changes which result when I raise both hands from this page is:

$$\begin{array}{cccccccccccc} a & \rightarrow & b & \rightarrow & c & \rightarrow & d & \rightarrow & e & \rightarrow & f \\ | & & | & & | & & | & & | & & | \\ p & \rightarrow & q & \rightarrow & r & \rightarrow & s & \rightarrow & t & \rightarrow & u \end{array}$$

where the top line represents the movements of the left hand, and the bottom line those of the right. A division of this movement-whole may take either this form:

$$\begin{array}{ccccc|ccccc} a & \rightarrow & b & \rightarrow & c & \rightarrow & d & \rightarrow & e & \rightarrow & f \\ | & & | & & | & & | & & | & & | \\ p & \rightarrow & q & \rightarrow & r & \rightarrow & s & \rightarrow & t & \rightarrow & u \end{array}$$

or this:

$$\begin{array}{ccccccccccc} a & \rightarrow & b & \rightarrow & c & \rightarrow & d & \rightarrow & e & \rightarrow & f \\ + & & + & & + & & + & & + & & + \\ p & \rightarrow & q & \rightarrow & r & \rightarrow & s & \rightarrow & t & \rightarrow & u \end{array}$$

The former we have termed 'Successive Division,' and the latter 'Simultaneous Division.' The corresponding reverse processes of building up more complex movement-wholes from simpler constituents are 'Successive Combination,' and 'Simultaneous Combination.'

Successive and simultaneous constituents are, of course, by no means independent of one another. In all activities where more than one muscle is contracting at a time, simultaneous constituents are necessarily present: whenever an action is spread out over a period of time, Successive Division is possible.

This paper confines itself to the relation between simultaneous constituents. Its problem is twofold:

(*a*) What happens in Simultaneous Combination? Does any movement, when combined with one or more other movements, lose, to some extent, its own character, and take on another character due to the combination? If so, what are the changes that take place?

(*b*) What happens in Simultaneous Division? Does a movement, when divided, resolve itself into a number of simpler movements, which bear exactly the same relation to one another in isolation that they did in combination? If not, what differences may we expect?

These questions are not open to any direct form of test, since no apparatus is available for measuring exactly the position and tension changes of all the muscles, joints and tendons in a complete and living member. Our only appeal, therefore, is to the relative efficiency of the movement before and after division or combination. An action is practised until a known degree of skill has been attained: it is then subjected to division or combination, and the efficiency of the agent again tested. If all other changes can be excluded, any loss of skill indicates a change in the structure of the original movement.

## II. Experimental.

### (*a*) *Apparatus.*

Fig. 1 is a diagrammatic sketch of the apparatus used. It consists of a flat wooden base (*A*), 55 × 27 cm., mounted on short rests. Pivoted on this board, in a horizontal position, are two strong wooden discs (*B*), so attached that they can be revolved about their centres, and locked in any

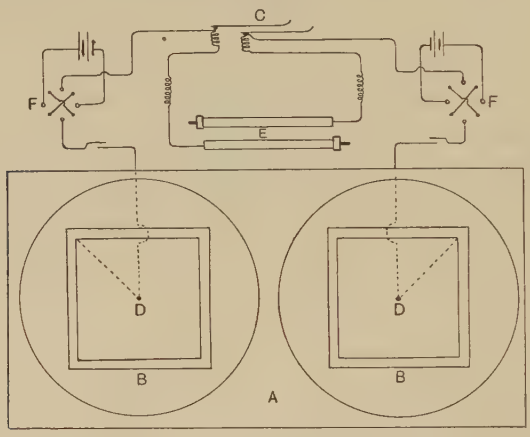



Fig. 1. Diagrammatic sketch of Figure-Tracing Apparatus.

*A* = Wooden Base. *B* = Wooden Discs. *C* = Time Markers.
*D* = Centre Bolts. *E* = Stiluses. *F* = Commutators.

position. These discs are pierced by holes which allow of the affixing of a variety of metal shapes. These consist of a number of geometrical figures, of differing shapes and sizes, cut in outline from $\frac{1}{16}$-inch brass. The strips forming the outline of the figures are uniformly 1 cm. in width. They are attached to the discs by means of screws sweated on their under sides.

Squares only were used in the experiments referred to in this paper. (Further experimentation involved various combinations of figures.)

The task is to follow around the brass squares with metal stiluses (*E*), in such a way that the contact between stilus-tip and figure is broken as seldom as possible. Both hands move in an anti-clockwise direction. Each break constitutes an error, and is registered by time-markers on a paper-tape kymograph. There is a separate circuit for each hand. The sides of the squares are parallel with the edges of the baseboard. The subjects are blindfolded.

Simultaneous Combination occurs when the right and left hands, having been separately trained in the tracing of the squares on the right and left sides of the apparatus respectively, are forced to perform these movements together. Division is represented by the reverse of this process.

### (*b*) *The groups.*

In order to answer the above questions at least six groups of subjects were necessary. With one or two exceptions they were all advanced students in psychology. The subjects were divided into two main groups. Group A commenced with the single-handed process (Simultaneous Combination), and Group B with the double-handed (Simultaneous Division). Group A returned to the single-handed after undergoing double-handed practice, and Group B returned to double-handed after the intervening period of practice on the single-handed process. A measure was thus gained of the amount of positive or negative assistance given by one type of practice to the other.

Groups A and B were further subdivided as shown in Table I. Each group was composed of four subjects. The distinction between Groups A-a and A-b, and between B-x and B-y was necessary in order to show to what extent there is a transfer of skill from practised right hand to unpractised left, and *vice versa*. Without a knowledge of this factor we cannot compare the performances of the A groups before and after double-handed practice.

The Control Groups, A-c and B-z, were rendered necessary by the fact that each subject's performance took from two to three hours. To avoid fatigue, the test was given in two sittings. Seven days elapsed between the sittings. The division, of course, was never made at any of the crucial points of change from Double to Single, and *vice versa*. But the mere lapse of time, however short, might be sufficient to invalidate our comparisons

between the original task and the return to it in the third part of the experiment. Hence the necessity for the Control Groups, where the lapse of time was the only factor intervening between the first and the third parts.

### (*c*) *Analysis of results.*

Figs. 2 to 7 give the results for the above-mentioned groups. The trials were grouped in sets of ten, so that each reading represents the total of ten trials. The total time taken (in seconds), and the total number of errors made by each individual in doing each set were found; and the average of these readings for the four subjects comprised in each group gave the figures on which the graphs are based. The Time Error (T.E.) score is merely a composite of the scores for time and errors.

There are four main problems involved in the study of our graphs:

(*A*) The behaviour of the curves at the first change, *i.e.* from Single to Double in the A Groups, and from Double to Single in the B Groups. This crucial point is marked by a vertical broken line in the graphs.

(*B*) The behaviour of the curves at the second critical point, where the subjects return to their original tasks. This is denoted in the graphs by the vertical dash-and-circle line.

(*C*) The amount of transfer of skill from the Single to the Double operation, and *vice versa*. This can be found only by a comparison of the levels attained by the A Groups and the B Groups in the various processes.

(*D*) The amount of transfer from one hand working singly to the other, also working singly.

### (*A*) *First change.*

Fig. 2, which represents the process of combination, shows a considerable rise in both time and error curves at the point of transition from the single to the double action. The same phenomenon is found, to an even more marked degree, in Fig. 3, which is the same process commenced with the left hand. There is nothing in these graphs, however, to indicate that any of this loss of skill is due to the breaking down of two movement-wholes and the necessity for the construction of a third distinct motor-whole. It might quite possibly all result from the inherently greater difficulty of the double-handed performance.

This is proved not to be the case by the records of the corresponding B Groups (Figs. 5 and 6). For here, although the change is from the inherently more difficult to the inherently less difficult action, there is still a sharp rise in the curve of errors at the point of transition.

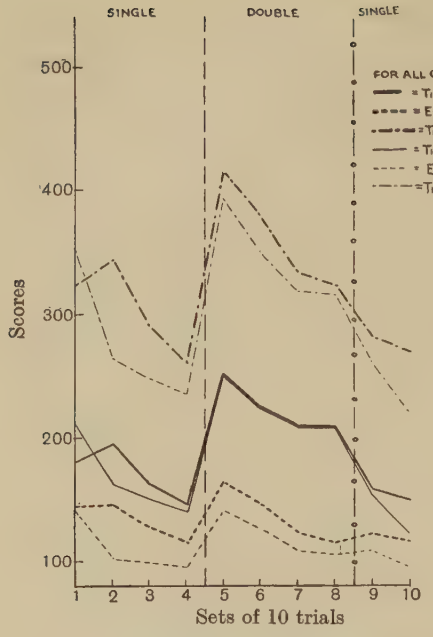
The improvement in the time curves is of no significance. It is practically meaningless to compare the *speed* of the single-handed movement with that of the double-handed, since, in the latter, there are considerable periods during which one hand remains stationary, whilst the other fumbles blindly in an attempt to find its bearings. There are two factors affecting the time curve in passing from Single to Double, or *vice versa*: (*a*) the loss of time caused by one hand having to wait for the other in the double-handed work, (*b*) the loss of efficiency which we anticipated would result from combination or division. Both factors work in the same direction in the A Groups (Single to Double), and in opposite directions in the B Groups (Double to Single). In the A Groups the average increase in time at the first change is 148 sec. (L.H.), and 146·5 sec. (R.H.). In the B Groups there is a decrease in time of 23·1 sec. (L.H.), and 27·4 sec. (R.H.). That is, factors (*a*) and (*b*) almost neutralize one another in the B Groups, proving that nearly half the decrease in speed at the change from Single to Double in the A Group is due to loss of efficiency resulting directly from the act of combination itself.

A comparison of the curves for the A and the B Groups, then, leaves no room for doubt that a transition from practised Single to unpractised Double, or from practised Double to unpractised Single always results in a great loss of skill. Since this loss occurs in both directions, there is no possibility of its being caused by the inherently greater difficulty of either action.

(*B*) *Second change.*

The results here are even more striking. We are dealing now with the subjects' return to the process with which they originally started. The points we are comparing are the ones immediately preceding the vertical broken line, and the ones immediately following the vertical dash-and-circle line.

It might well be expected that a subject already trained in the single-handed process would gain in single-handed skill from a lengthy period of practice with the two hands working together. Even more confidently would one predict that single-handed practice could do nothing but add to the skill already attained in the double-handed performance; for we approach most of our complex activities by preliminary practice on their analysed-out constituents.




| A → | SINGLE | followed by DOUBLE | followed by SINGLE |
|---|---|---|---|
| A-a (Fig 2) | R.H (40) L.H (40) | (40) | R.H. (20) L.H (20) |
| A-b (Fig 3) | L.H (40) R.H (40) | (40) | L.H (20) R.H. (20) |
| A-c (Fig 4) | R.H. (40) L.H (40) | NIL | R.H (20) L.H (20) |
| **B →** | **DOUBLE** | **followed by SINGLE** | **followed by DOUBLE.** |
| B-x (Fig 5) | (40) | R.H (40) L.H (40) | (20) |
| B-y (Fig 6) | (40) | L.H (40) R.H (40) | (20) |
| B-z (Fig 7) | (40) | NIL | (20) |

C (with vision) as for A-b. D (with vision) as for B-y.

E (with altered mental attitude) as for B-y.

*Table I.* — Showing Grouping. (R.H. = Right Hand. L.H. = Left Hand. e.g. R.H (40) = 40 trials with right hand).

Fig. 2. Group A-a.
Single (R.–L.)—Double—Single (R.–L.).


Fig. 3. Group A-b.
Single (L.–R.)—Double—Single (L.–R.).

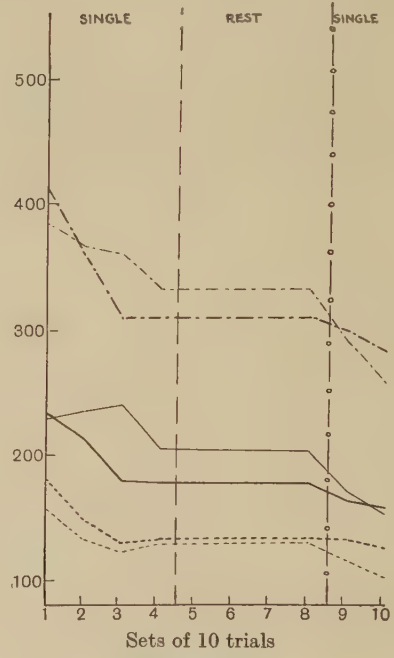



Fig. 4. Group A-c.
Single (R.–L.)—Rest—Single (R.–L.).

Exactly the reverse appears to be the case in the results we have obtained. There is not a single case in all the averages for Groups A-a, A-b, B-x and B-y in which the return to the original process, be it Single or Double, is not accompanied by a marked rise in both time and error curves. In the case of the return to the single-handed process (A Groups), the average T.E. of the left hand rose from 268·5 to 294·3, and of the right from 247·2 to 274·8. The corresponding figures for the double-handed process (B Groups) were: L.H. 302·6 to 344·2; R.H. 291·1 to 319·7.

We cannot conclude, however, on the basis of these results alone, that there is any interference between the performance of the single and the double action. The loss of skill in the third part of the experiment may be due simply to the time that has elapsed between the first and the third parts. Hence the need for the Control Groups, A-c and B-z, in which the second part of the experiment was omitted altogether. Any loss of skill in these groups must be due to fading alone.

But the results show no loss: there is a decided gain in the case of both the single and the double processes (Figs. 4 and 7). In the right hand of Group A-c the T.E. curve shows an average improvement of as much as 44 points. This does not necessarily mean that there has been any 'consolidation' during the seven days' rest period, but only that the improvement during the first set of ten trials after the rest more than equals any loss of skill due to fading.

The conditions were the same for the Test Groups as for the Control Groups. The only factor present in the former and absent from the latter was the interpolation of a different activity between the first and the third parts of the experiment, so that all the difference between the fall in efficiency in the Test Groups and the rise in efficiency in the Control Groups must be due to the 'interference' of the double process in Groups A-a and A-b, and of the single process in Groups B-x and B-y.

(*C*) *Transfer of skill between single and double operations.*

In spite of this, the A Groups, who came to double-handed work after practising single-handed, began the double-handed task at a higher level of performance than did the B Groups, who had had no preliminary single-handed practice. The B Groups, on the other hand, had the advantage in the single-handed task, which, with them, followed a period of double-handed practice.

Table II shows this clearly. The figures for 'Single Left' were got by subtracting the T.E. scores for Group B-y from those of Group A-b. 'Single Right' was got by subtracting the T.E. scores for Group B-x

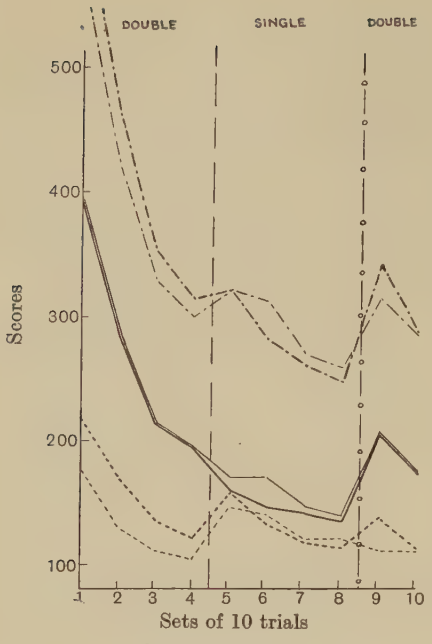



Fig. 5. Group B-x.
Double—Single (R.–L.)—Double.


Fig. 6. Group B-y.
Double—Single (L.–R.)—Double.


Fig. 7. Group B-z. Double—Rest—Double.

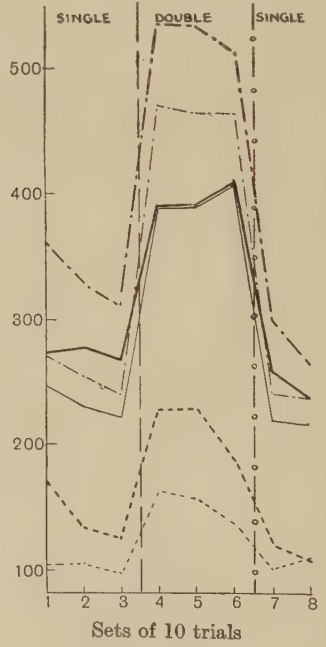



Fig. 8. Group C. With vision.

from those for Group A-a. Transfer from Single to Double presents a more difficult problem. The double action in the A Groups is never begun until both right and left have practised singly; and it is then impossible to say how much each has contributed to the final amount transferred. The nearest we have been able to do, in computing the 'Double' column is to subtract the average Double T.E. of the A Groups from the T.E. of the B Groups.

Table II. *Showing amount of transfer of skill between single and double operations.*

| | Single | | Double | |
|---|---|---|---|---|
| | Left | Right | Left | Right |
| Initial advantage of practised group | 139·0 | 36·5 | 57·8 | 41·1 |
| Final advantage of practised group | 30·3 | −24·9 | −55·7 | −55·2 |

A 'practised group' is one which comes to the single performance after practising the double, or to the double after practising the single.

An inspection of the T.E. marks for the first set of practices of the A Groups (Single to Double) and the fifth set of the B Groups (Double to Single) shows that those who came to the single performance after practice on the double had a marked initial advantage over those who started immediately with the single. The average gains are: L.H. 139 points; R.H. 36·5 points.

When we compare the fourth practice period of the A Groups, however, with the eight of the B's, we find that further practice has reduced this advantage of the 'Doublers' to: L.H. 30·3 points; R.H. — 24·0 points.

There is every indication that, if practice had been pushed a little farther in both groups, this advantage might have finally disappeared altogether in the left hand as it has in the right, even to the point of being transformed into a disadvantage.

However this may be, our results are sufficiently definite to lead us to suppose that the initial effect of previous double-handed practice is to give considerable help in the single-handed process, but that this advantage gradually diminishes as the single practice proceeds, until, possibly, it vanishes entirely.

This phenomenon is shown in a much more striking fashion in the 'Double' column. The A Groups started their double-handed performance with an advantage over the B Groups of 57·8 points of T.E. in the L.H., and 41·1 points in the R.H. After four sets of double-handed practice, the position is reversed: the B Groups now have the advantage, 55·7 points in the L.H., and 52·5 in the R.H. This means that the long

period of single-handed practice has only served, finally, to hinder the acquisition of skill in the double-handed performance. Important practical conclusions would follow from such a principle.

(*D*) *Transfer of skill as between single hands.*

The question of the amount of transfer of skill from one hand to the other is of interest to us here only as it serves to throw light on the nature of the transfer between the single- and the double-handed operations. Table III shows, in parallel columns, the amounts of transfer from Double to Single, and from one hand to the other.

Table III. *Showing amount of transfer of skill between single and double operations, as compared with transfer from hand to hand.*

| | Transfer from Double to Single | | Transfer from hand to hand | |
|---|---|---|---|---|
| | | T.E. | | T.E. |
| Initial | Double to Left | 139·0 | Right to Left | 146·8 |
| | Double to Right | 36·5 | Left to Right | 29·5 |
| | Total: Double to Single | 175·5 | Total: hand to hand | 176·3 |
| Final | Double to Left | 30·3 | Right to Left | 15·7 |
| | Double to Right | −24·9 | Left to Right | −27·2 |
| | Total: Double to Single | 5·4 | Total: hand to hand | −11·5 |

The figures for 'hand to hand' were obtained as follows:

*Right to Left*: (initial) T.E. of 1st set Single Left of Group A-b minus 1st set Single Left of Group A-a (final). The 4th sets instead of the 1st were used.

*Left to Right*: similar to 'Right to Left,' except that Group A-b is now subtracted from Group A-a.

The great advantage of the left hand over the right in Table III is only apparent. It is due largely to the fact that the subjects in Group A-a happened to be rather above the average in ability, and those of Group A-b a trifle below the average.

Table III shows a remarkable correspondence between the amount of transfer of skill from Double to Single, and the amount occurring on the passage from hand to hand. Each hand gains almost the same initial advantage whether it is preceded by the double-handed action or by a period of practice with the other hand. As in the case of 'Double to Single,' the initial positive transfer from hand to hand diminishes rapidly with further practice, even to the point of being converted into a definitely negative transfer in 'Left to Right.' 'Final Right to Left' would probably have shown the same negative transfer had it not been for the natural superiority of the A-a subjects mentioned above.

The similarity of the two sets of figures would lead us to suppose that exactly the same factors are transferred from the double to the single action as from hand to hand. In that case, it would seem to follow that

those factors are of a general rather than a specific nature, a matter of mental attitude rather than of habits of manipulation. But the evidence on this point is not final.

Nevertheless, our results certainly justify us in concluding that there is an initial positive transfer of skill both between single- and double-handed action, and between right- and left-handed action, and that this initial advantage is rapidly lost with further practice. The final result is generally an actual disadvantage for the people who have come to one action after a previous practice-period on the other.

### (*d*) *Negative experiments.*

Any attempt to explain this interference between the single and the double operations must be based on a study of the conditions under which the phenomenon does not occur. Accordingly, some of the tests already described were performed again with the following modifications: (A) With vision, instead of blindfolded. (B) With an altered mental attitude.

(A) *With vision.*

Two groups (C and D), of four subjects each, were used. Group C corresponded to Group A-b, *i.e.* it proceeded in the order: Single (Left then Right)—Double—Single (Left then Right). Group D resembled Group B-y in the order: Double—Single (Left then Right)—Double.

As a result of the eyes being left uncovered, the task became much easier. The subjects, therefore, were made to go three times round the figures for each trial. Thirty trials were given in each process, instead of forty. The results are given in Figs. 8 and 9.

*First change.* Group C (Fig. 8), like its 'blindfold' counterpart, A-b, shows a rise in the T.E. curve on passing from the single to the inherently more difficult double action. Unlike Group A-b, however, Group C shows no tendency to improve on the 'Double' with further practice, thus leading us to assume that the rise in T.E. just mentioned was due solely to the greater difficulty of the double task.

The changes in Group D (Fig. 9) are unequivocal. There is an unmistakable improvement in all curves upon passing from the double to the single operation. A more marked difference than that between the behaviour of Groups D and B-y (Fig. 6) in this respect it would be difficult to find. The difference can be ascribed only to the influence of vision, which seems to have robbed the process of Simultaneous Division of all its harmful effects.

*Second change.* The results on the return to the original process are exactly the reverse of what the blindfold experiments would lead us to expect. Instead of the old interference between Double and Single, we find, in both Groups C and D, an actual diminution of T.E. resulting from the interpolation of Double between Singles, and Singles between Doubles.

In general, we may state, the effect of Simultaneous Combination and Division upon any skilled action is harmful if vision be excluded. If vision be permitted, there is a tendency for this harmful effect to disappear. Further, without vision, there is an interference between single and double actions; with vision, there appears to be a positive transfer from one to the other.

We can explain this difference only by reference to the introspections of the subjects themselves. They are quite consistent on this point. All the subjects in these two groups recorded that, in the double operation, the eyes fluctuated rapidly between the two hands. The experimenter's observations revealed that, normally, the subjects' eyes rested about three times on each hand in the course of one side of the square. This fluctuation of the eyes, the subjects complained, brought about a corresponding fluctuation of attention, which prevented the movements of the two hands from ever becoming one action for consciousness. At no time were the two actions controlled by one mental 'fiat.'

It is not difficult to understand, therefore, why there was so little dislocation in passing between the double and single actions, and no interference between the two upon returning to the original operation. The double action is double only in the sense that two separate and independent single actions are taking place at the same time. To some extent they are synchronized, but there is none of that intimate fusion and loss of individuality which, we shall see later, is the mark of skilled double action, as performed without vision. (It is interesting to note that, from the subjective standpoint, the effect of vision on the performance was to convert it into a case of something approaching *successive* combination.)

We must assume, then, that the characteristic phenomena of combination and division, found in the previous experiments, are, in some way, connected with this fusion of two simultaneous movements into a mental and physiological unity. A further experiment was performed, therefore, in which this fusion of the constituents was voluntarily inhibited by the subjects.

(B) *With an altered mental attitude.*

Six subjects were used. They constituted a single group (E), and performed the following sequence: Double—Single (Left then Right)—Double. Group E thus corresponds to Group B-y, the only difference being that these present subjects were required to think of the two hands as performing separate and independent actions, and were instructed to suppress instantly any tendency towards a fusion of the two movements into a mental whole. The results are given in Fig. 10.

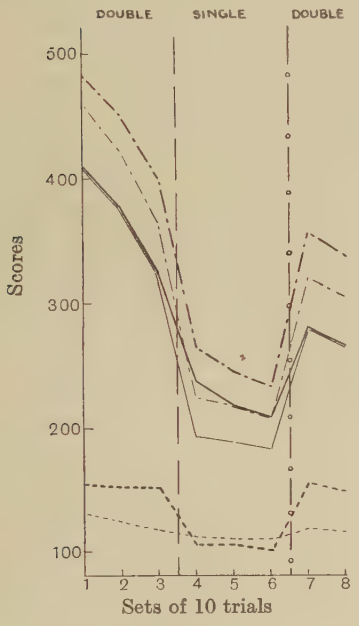



Fig. 9. Group D.
With vision.


Fig. 10. Group E.
With altered mental attitude.

It can be seen at a glance that Group E is of the order of Group D (Fig. 9) rather than of Group B-y (Fig. 6). At the first change there is an improvement in the error curve, and there is no interference upon the return to the original process. By preventing the fusion of the two actions into a unified cognitive and muscular whole during the double-handed practice we have again eliminated the phenomena which, in the A and B Groups, were connected with Division and Combination.

### III. Consciousness and the acquirement of skill.

But how is the 'fusion' experienced by the subject himself? Our introspective evidence on this point throws considerable light on the general relation of consciousness to the acquirement of skill. Writers on this subject are divided into two schools; first, those, who, like Pear[1], hold that concentrated attention is the secret of success in the muscular sphere, and, secondly, those who follow Koffka in asserting that the direction of consciousness "has no influence upon the efficiency of learning[2]."

Our introspective notes lead us to believe that this disagreement arises from the failure to realize that the direction and nature of consciousness varies continuously during the learning process. We have, therefore, distinguished two relatively distinct stages in the acquisition of any skilled activity, each stage offering a different problem of attention.

#### (*a*) *Learning stage.*

All subjects, without exception, reported that, in beginning the double-handed procedure, they were forced to fluctuate their attention between the two hands. The rate of fluctuation depended upon the degree of initial skill. Objectively, this was noticeable in the alternate spurts of the two hands. This fluctuation would appear to be necessary in order to enable each hand to develop a preliminary rough 'movement-pattern' of its own in the focus of consciousness. Such 'patterns' seem to be incapable of developing on the margin of consciousness. The only possible solution in such cases is a rapid alternation of the focus between the two constituents.

Speaking in terms of Spearman's "Principle of Mental Energy," we may say that each hand demands the majority of the available mental energy in order to organize its own 'pattern' sufficiently to enable it to function at all. The fluctuation of the focus of consciousness is a fairly successful compromise between the rival claims of the two hands.

Two intermediate steps were noticed by many subjects in the progress from the 'learning stage' to the 'expert stage.' The first is what we have termed the stage of 'semi-crystallization,' when the focus of consciousness, in its fluctuations, lingered on one of the hands to the relative exclusion of the other. The total action then tended to 'crystallize out' around the dominant hand. The other hand simply followed. Such an

[1] *Skill in Work and Play* (London, 1924), pp. 37–45.
[2] *The Growth of Mind* (London, 1924), p. 259.

organization about one constituent was always of a temporary nature, and was broken down by an error in the neglected hand. But it seems to represent a premature effort after some kind of unity between the constituents.

The second transition stage was noticed by only some of the subjects. When each hand had become so far automatic as to demand no longer the whole of the available mental energy, it was found that a bifocal consciousness evolved. There was no alternation of attention, and yet one had a clear focalized consciousness of each hand as more or less independent of the other.

(*b*) *Expert stage.*

Focality of consciousness, we have seen, is the essence of the 'learning stage.' The 'expert stage,' on the other hand, is characterized by diffusion of attention, non-focality. The state of mind is best understood by reference to extracts from subjects' introspections: "Apparently attend to nothing at all. Work best in a vague, uniform, even slightly dazed state of mind." "A blank, mechanical feeling." "Let the hands swim." "Hazy, like the feeling when you are just going to sleep." "Like the early stages of hypnosis. The general level of consciousness is low."

It was found that this state of non-focality of consciousness always accompanied the most efficient performances. In only a few of the more proficient subjects did it show any permanence. The rest experienced short periods of diffused consciousness which were rapidly brought to a close by an error in either hand. In the Vision Groups it was never reported. But there was general agreement amongst all the other groups that the acquirement of real skill in the double-handed performances was coincident with the oncome of this state of non-focality of consciousness.

This seems to fit in with a research of Spearman's in which blindfolded subjects were required to localize a tactual sensation on a passively moved limb. When attention was focused on the sensations the error was twice as large as it was with non-focalized consciousness. Spearman explains this by assuming that, in the non-focal state, more mental energy is liberated for 'unconscious articular factors.' A similar explanation might cover the present case[1].

The efficiency of the non-focalized 'low-pressure' consciousness in routine motor performances is what we might be led to expect from such extreme cases as walking and writing, which are carried on almost un-

[1] See Spearman, *Die Normaltaüschungen in der Lagewahrnemung*. Also Spearman, this *Journal*, 1905, p. 287.

consciously, and which are liable to suffer if they become the centre of attention.

A further interesting stage was noted by some subjects, a stage in which there was some slight degree of focality, but in which the focus of attention rested not on any part of the action, but on some external object, such as the hum of the motor that drove the kymograph, or upon the act of whistling or counting. Such subjects reported that the pure non-focal state was the most efficient, but that the tendency to focalize was too strong to resist for long, and that, sooner than ruin the unity of the whole action by focalizing upon any constituent, they chose some external object of a fixed or rhythmic nature for the focus.

## IV. Summary and conclusions.

(1) Division of a movement-whole into its simultaneous constituents results in a loss of efficiency in the performance of those constituents.

(2) Conversely, a combination of relatively simple movements into a movement-whole results in a loss of efficiency.

(3) A still further loss accompanies the return to the original performance, after either division or combination.

(4) From (1), (2) and (3) we have concluded that constituent movements are essentially different according as they are performed in combination or in isolation.

(5) There is an initial positive transfer between the single and the double operations, but further practice converts this into a final negative transfer.

(6) There is the same amount of transfer, initial and final, from Double to Single as from hand to hand. From this we have concluded that the transfer is general rather than specific in nature.

(7) There are two main stages in the process of acquiring skill: (*a*) *Learning stage.* Characteristics: focality and fluctuation of consciousness. (*b*) *Expert stage.* Non-focality of consciousness.

(8) In so far as any factor prevents the formation of this non-focalized consciousness around a movement-group there will be no 'fusion' of the constituents, and the phenomena noted under (1), (2) and (3) will not occur.

(9) Non-focalized consciousness is the ideal to be aimed at in the acquisition of mechanical skill, but, in practice, it will often be found necessary to satisfy the almost irresistible tendency towards focalization by the introduction of some experience relatively external to the central task, to serve as the object of this focus.

(10) By deduction from the above principles it would appear that the 'whole' method of learning a muscular habit is preferable to the 'part' method, but further detailed research is necessary in order to discover at exactly what degree of complexity in the movement the disadvantages of the 'whole' method outweigh its advantages.

(*Manuscript received* 19 *June*, 1929.)

# CHILDREN'S THINKING[1].

By VICTORIA HAZLITT.

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## I. The theories of Piaget.

One of the most interesting and important works on child psychology that have appeared during the last few years is that of Piaget on the language and thought of the child. I propose to outline very briefly some of Piaget's views, and to give an account of work that I have been doing which bears upon them.

For Piaget the most fundamental fact about childish thinking is that it is egocentric. To this fact he ascribes a number of peculiarities which he holds differentiate qualitatively children's thinking from that of adults. He comes to his conclusion with regard to childish egocentrism from a study of children's spontaneous chatter in the classrooms of the Maison des Petits. He found that children of 3 years of age made 28 per cent. more remarks addressed to no one in particular than children of seven. He holds that the social life of the child does not begin until about 7 years of age, and that before that time, on account of his egocentrism, his thinking processes are handicapped in specific ways, the chief and most fundamental of which is his inability to see relations. For instance, such relations as that of brotherhood are misconceived by the child because he can only see things from his own point of view—even though he knows that he has a brother X, he cannot realize that X must therefore have a brother, viz. himself. Piaget shows that relations such as that of excep-

[1] Read before the Psychology Section of the British Association in South Africa, July 1929.

tion (cutting out an individual or group from a generalization) cannot be expressed verbally until seven or later. This inability to deal with relations leads, according to Piaget, to certain peculiarities of childish thinking, such as syncretism—the tendency to place side by side things that should have a more significant relation; transduction—the tendency to generalize from one example.

## II. CRITICISM OF THESE THEORIES.

It will be seen that Piaget's theory implies that the child's thinking differs fundamentally and in kind from that of the adult. What he says with regard to egocentrism and the child's lack of ability in thinking relations before the age of seven would, if taken literally, imply that children at the earlier period are scarcely capable of thinking. Now such a view runs entirely counter to common sense. If the child's thinking is really distorted in the way Piaget suggests, how comes he to master his world as well as he does in the first few years? Further, individual observations of quite young children often afford evidence of a logic and a grasp of relations wholly unexplained on Piaget's view. When we examine Piaget's work to discover what led him to his obviously false conclusions, we find two assumptions for which he gives no evidence. Indeed, he himself admits in different places that they are not justified, but still assumes them in drawing his conclusions. The first is that thinking can be identified with ability for verbal expression. The second is that the matter of thought is indifferent to the process; that if, for instance, a child cannot think a relation in regard to one subject-matter, he cannot think it in regard to another. In the study which I am going to describe, I tried to collect evidence bearing on the validity of these assumptions, and to discover whether children's thinking differs from adult thinking in the radical manner suggested by Piaget, or whether the difference is one of degree.

## III. EXPERIMENTAL INVESTIGATIONS.

### (*a*) *The problem of making an exception.*

With these ends in view I tried to invent ways of making the child perform practically some of those operations of which Piaget says that, judged by his powers of verbal expression, he is incapable. I shall deal first with the problem of making an exception. It is quite easy to verify Piaget's claim that few children under seven use spontaneously or *can* use correctly the word, 'except.' The following experiments were an

attempt to get beyond this to the beginning of the child's ability to make an exception practically. With eighty-eight children ranging from three to seven years of age a Russian egg was used. It consisted of a number of differently coloured eggs shut, the smaller within the larger, into one large blue egg. The series was opened out and laid in front of the child, and he was told to put them all back except the green. All the work was individual. The youngest children tested, and dull, older children did not take up the problem at all, they simply played. Slightly older ones took up the problem and packed the eggs back, but failed to except the green, either because they did not understand the instruction, or because it dropped out of mind before they came to the stage for observing it. When they failed to carry out the instruction, although obviously trying to do so, the experimenter gave them another trial, avoiding the use of the word 'except.' "Put them all back but not the green one." Several who had previously failed succeeded to this form of instruction. Now on Piaget's view these children could not think an exception and yet, in what was for them a complicated and absorbing process, they managed to make an exception practically. Is it then legitimate to assume that the whole core of thinking lies in being able to attach the given process to the word 'except' rather than to any equivalent that the child may have?

The youngest child to solve the problem to the word 'except' was aged 3 years 4 months. When I found that very young children could solve this problem I tried them with another in which a group instead of an individual had to be excepted. The material consisted of twelve cards in different colours. Four of them were ornamented with a moon and a star. The child was told to put them all down on a tray in front of him except those that had a moon and a star. As before, the youngest merely played with the material and did not attempt to solve the problem. Another group divided the ones to be excepted from the others but applied the generalization to the exceptions. The youngest child to carry out the instructions perfectly was aged 4 years 8 months. It is obvious that this problem requires a much greater effort than the previous one, as the instruction has to be applied to a series of cases. Finally, I gave a problem that involved making an exception—although no verbal instruction to that effect was given—and then formulating the process. The material consisted of coloured and ornamented cards, mixed in with twelve black of the same size and five smaller black cards. The experimenter told the child that some of the cards were called *K*, and that he was to watch the experimenter put the pack down one at a time and to notice to which she

called out the name *K*. The experimenter called out *K* as she put down each of the large black cards in rows on the child's right. The others, including the small black, she put on his left without giving them any name. All the cards were then collected and, after being shuffled, they were dealt out one at a time and the child was told to call out *K* to the right ones.

As with the other problem, there were some children who did not seriously attempt a solution. Others who attempted it failed to except the small blacks, and called all blacks *K*. In some cases these children succeeded when the whole process was repeated, in others they did not. All the children who succeeded in solving the problem were asked how they would tell someone else which were the *K*'s. The following is typical of the conversation that would ensue with children under five.

*Child.* The blacks are *K*.
*Experimenter.* All the blacks? Yes.
Is this small one *K*? No.
Is it black? Yes.
Then are all the blacks *K*? *Yes.*

These children could make the exception practically, but they could not formulate it at all. At the next stage they would formulate it in one of these ways: "The large blacks are *K*'s, the little blacks are not"; "The blacks are *K*, but not the little ones." It was only bright children of seven or eight who would say, "All the blacks, except the little ones, are *K*." With the children who had not reached this stage and who could only express the matter in one of the alternative forms that I have mentioned above, I took a further step and gave them the sentence, "All the blacks except the little ones are *K*" with the word 'except' omitted, and asked them which of the three words, 'although,' 'except,' 'but not,' would make the best sense. The youngest children would agree to all the alternatives. Slightly older they favoured 'but not.' Finally, a large group of children who did not use the word 'except' spontaneously chose it unhesitatingly from the alternatives offered them.

This series of results from the success of the youngest child to except the green egg to the seven year old's spontaneous use of the word 'except,' gives some very slight idea of the enormous number of integrations that must take place before a relation which can be thought can be put into the accepted verbal form.

### (*b*) *The problem of generalization.*

All modern writers on children's thinking comment on their errors of generalization. A great many, and Piaget is one of these, speak as though

in this respect the child's thinking might be sharply contrasted with that of the adult. I have begun to collect data on how children tend to generalize. The first problem involved in generalization is that of seeing a common element in a number of things. With the youngest children I used simple concrete material. One series consisted of four small trays holding respectively:

Dog and bird.
,, pig.
,, cow.
,, sheep.

The other series held:

Matchbox and man.
,, hunter.
,, can.
,, goose.

The two series were used separately; both were used with all the children to ensure as far as possible that any failure that there might be was not due to the particular material used. The experimenter would draw the child's attention to the four trays and ask, "What is there on all the trays?" If this did not succeed, she would vary the wording, "What is the same on all the trays?" "What have all the trays got?" This last proved to be much the most intelligible form to the children. Very few children under five could pick out the common element in either of the series. The dog seemed to be much easier for them than the matchbox which, from its familiarity, apparently, fell into the background in relation to the other object on each of the trays. No child under four years solved the matchbox problem. The youngest to solve the dog problem was 3 years and 2 months. When a child failed with either problem I asked him to name the objects on each tray in turn, so that the common object would be named four times, each of the others only once. Immediately the child had named them all I would say, "What is there then on all the trays?" No child that was unable to name the common object was helped by this repetition of the names. This seems to show that their failure was not due to inattention or lack of method in their way of attacking the problems. What then was the difficulty? There seems no doubt that the realization of sameness in difference is an experience that comes more or less suddenly to the child, and that in some it does not come much before three years. Difficulties with regard to it often show themselves in the early stages of numeration, and one observer remarks on her son's wonder and excitement at two Vim tins standing side by side. He kept on drawing attention to their being alike. This was at about

two years of age. If the recognition of the sameness of two things does not come much before this, to see what is the same in as many as four different things must be a very great effort to the three-year old. Further study of this problem would be interesting. The evidence to hand shows that recognition of a common element is possible for such very young children, that any disability that they may have in generalizing at the ages of which Piaget speaks cannot be due to an inability to see a common element.

Another important process in generalization is that of extending what is known to be true of certain cases to others which are not exactly the same, but which resemble them in various ways. For the study of this, material of a very simple nature and of a kind that would be equally unfamiliar to all was used. It consisted of thirty cards of different colours, some plain, others marked with gilt moons and stars in different arrangements. The child was told that two which were shown to him were called *A*. One of these was blue with a moon and star upon it, the other was yellow and similarly decorated, except that the moon and star were not in exactly the same relative positions. He was then asked to pick out all the other *A*'s from a pack, none of which was identical with the blue or the yellow as to the arrangement of the moon and star. The youngest children to attempt this picked all the cards with marks of any kind, omitting only the cards that were quite plain. Slightly older children chose only those with a moon and star, rejecting all the plain ones and all with such marks as two moons or three stars. All these children chose without any reference to the colour of the background. The youngest child to show any change on this procedure was an exceptionally intelligent boy of six, who chose only blue and yellow cards with a moon and star. The brighter children of seven and eight took into account the relative position of the moon and star, and tried to choose only those in which it was the same as in the two samples. The various bases of generalization that the children mentioned when they were questioned about their choice may be summarized in the order of their appearance with age thus (beginning with the youngest):

(1) Gilt marks of any kind on any background.

(2) Moon and star in any position on any background.

(3) Moon and star in any position on blue or yellow.

(4) Moon and star in roughly the same relative position as in sample on any background.

(5) Moon and star in roughly the same relative position as in sample on blue or yellow.

No child tested raised any difficulty with regard to the problem, although the older ones were obviously puzzled as to what to take for the basis of generalization. They were much longer than the younger children in choosing the cards. Of the whole group it is true that there was present in the mind of the child some basis for generalization. The youngest took the broadest basis, and the oldest the narrowest. Incidentally, it is interesting to note that difference in colour of background was the thing that affected choice least, and that the children who did take it into account were the oldest or the most intelligent.

## IV. Conclusions.

Writing some years ago, Prof. Burt declared that he found no evidence of any specific process of thought that could not be performed by a child of seven. I should like to suggest that, from the evidence that is accumulating, it seems likely that there is no age limit in relation to the processes of thinking, beyond that imposed by lack of experience. If Piaget were right when he says that childish egocentrism makes certain processes of thought impossible before seven years of age, how could we account for the evidences of these processes at three and earlier? The truth seems to be that the child's egocentrism is largely due to his lack of experience—his world is so often made by others to foster in him the notion that he is the centre of it. His lack of experience makes him unable to see relations, and his inability to see relations makes him egocentric. It may be noted in parenthesis that Piaget's assumption that children are naturally non-social in their thinking until the age of seven is being questioned by psychologists who are working with young children. Baldwin and Stecher note social conduct and a degree of co-operation as early as two years of age in circumstances that favour and give ample opportunity for it. Very few children under five, and still fewer under three, have such opportunities, which can only come to the child who is with others at approximately the same level of development. When he is with children who are much younger or much older, the relations cannot be reciprocal, and he does not derive the benefits that he would from a social environment.

A further argument against the view that there is any radical difference between the thought processes of the child and of the adult, comes from the fact that when the adult has to think about wholly unfamilar material he makes the same mistakes as the child. He even does it with familiar material if he has not been obliged to concern himself with it previously. For instance, Piaget tells how children in ac-

counting for the displacement of water by objects of differing size and density sometimes give the weight as the cause, and sometimes the size, without bringing the two into relation or seeing any difficulty in giving different causes for the same phenomenon. I repeated his experiment with some non-scientific adults and found exactly the same results. They were no more disturbed by the contradictions in their answers than were the children. Piaget's picture of a striking difference between adult and childish thinking is, I believe, due to an over-valuation of verbal expression as a measure of thinking, and an exaggerated view of the logicality of adult thought.

(*Manuscript received* 18 *October*, 1929.)

# THE QUANTITATIVE ESTIMATION OF THE SENSATION OF COLOUR.[1]

By T. SMITH.

(*From the National Physical Laboratory.*)

The recent papers by Dr L. F. Richardson and Mr R. S. Maxwell on The Quantitative Estimation of the Sensation of Colour are an attempt to controvert William James' statement "To introspection, our feeling of pink is surely not a portion of our feeling of scarlet" by producing quantitative estimates of the relation between white, pink, and scarlet. My purpose in this note is to suggest that the enquiry they conducted was not suitably framed for the elucidation of this question, and that the results obtained may have a different significance.

Reliable views on psychological questions of this type are not readily obtained; the widespread study of physics makes it increasingly difficult to find numbers of intelligent persons able to analyse their sensations without reference to the known nature of the exciting stimulus. My own introduction to physics was sufficiently late for me to remember my surprise on learning that pink is a desaturated red. Up to that time I had thought of a relation between pink and scarlet as little as one between green and blue or between yellow and blue. To a large extent these unsophisticated views remain: browns do not in general appeal to me as dark shades of orange; lilac and mauve sensations belong to a class of their own, having no obvious connection with either red or blue. I have noticed the same thing in children: they will say for example that the marks made by certain crayons are of a quite different colour from those of the chalks themselves, though with other colours the corresponding saturation contrasts excite no comment.

It appears to me that Dr Richardson and Mr Maxwell have begged the whole question by calling for a quantitative answer. The only pertinent question is whether for each individual the 'feeling' of pink is or is not a portion of the 'feeling' of scarlet. The very fact that in conducting these experiments it was necessary for the investigators to explain that there is no correct answer suggests that for many of the subjects the two feelings are not associated. It is also important to

[1] See this *Journal*, xx, 1, 1929, and xx, 2, 1929.

realize that their fundamental question was put in such a way as to ensure an answer apparently unfavourable to William James. Any mark on a line connecting white and scarlet would serve this purpose.

This is not the occasion on which to discuss the very complex factors introduced by taking the relative distances of three marks rather than the order of a greater number of points as the basis of computation. The intention in these experiments is to induce the subject (i) mentally to estimate the number $y$, where

$$\frac{y}{100} = \frac{f(\gamma) - f(\alpha)}{f(\beta) - f(\alpha)},$$

and $f(\alpha)$, $f(\beta)$ and $f(\gamma)$ represent the measures of sensations evoked by stimuli $\alpha$, $\beta$, $\gamma$ (the existence of some innate sensation scale being assumed), and then (ii) to guess which point of a line of given length is distant the percentage $y$ of the whole length from one end. The process is evidently not simple. That the position of the marks were somewhat widely scattered is not surprising. If any feature calls for explanation it is the definite character of the curves, resembling hyperbolas, connecting the means given in Mr Maxwell's paper.

Pressed to compare sensations qualitatively distinct, it is natural that we should, without necessarily realizing the criterion adopted, seek for some measurable attribute which all the sensations possess in common, though in different degrees. In experiments on sight the attribute of brightness is very striking, and exists quite apart from colour. It is not difficult to make approximate estimates of relative brightness, though precise measurement in the presence of colour differences is difficult. For example, we have no difficulty in forming very fair estimates of the relative brightnesses of red, orange and green railway signal lights. It is not improbable that, in the absence of experience in colour mixture estimations, the opinions given in these experiments actually relate to estimates of relative brightness. If this be assumed, we can determine from well-established laws what the character of the observed curve should be. Let $\alpha$ be the brightness of the white surface, $\beta$ that of the red surface, $\gamma$ that of the pink when the fraction $x$ of the total angle is red, the remainder being white. Then

$$\gamma = (1 - x)\,\alpha + x\beta,$$

and $f(\xi)$ is to be interpreted as $\log \xi$. Thus we have the theoretical relation

$$\frac{y}{100} = \frac{\log \gamma - \log \alpha}{\log \beta - \log \alpha} = \frac{\log \gamma/\alpha}{\log \beta/\alpha} = \frac{\log (1 - x + x\beta/\alpha)}{\log \beta/\alpha}.$$

It was expected that $\beta/\alpha$ for the samples used would lie between 1/5 and 1/10. Trial with $\beta/\alpha = 0{\cdot}14$ gives the following results, with which Mr Maxwell's means are compared. All the figures are expressed as percentages.

| $x$ | $y$ calculated | $y$ observed (Maxwell) |
|---|---|---|
| 0 | 0 | 0 |
| 20 | 9·6 | 10 |
| 50 | 28·6 | 27 |
| 80 | 59·2 | 58 |
| 90 | 75·6 | 76 |
| 95 | 86·4 | 91 |
| 100 | 100 | 100 |

The agreement of the observed means with the values calculated on the supposition that relative brightnesses are being estimated is closer than could have been expected from such a series of experiments. This comparison of course does not entitle us to infer that the estimates were in fact based on brightness comparisons, but it suffices to show that these investigations do not establish any facts tending to discredit William James' opinion.

(*Manuscript received* 18 *November*, 1929.)

# THE QUANTITATIVE MENTAL ESTIMATION OF HUE, BRIGHTNESS OR SATURATION.

## A REPLY TO MR T. SMITH'S CRITICISMS.

By L. F. RICHARDSON and R. S. MAXWELL.

There seems to be a fairly general agreement that the classification of 'formless visual sensations' (by which we mean uniform expanses of colour, white, grey or black) requires three independent qualities. Parsons[1] names them (1) hue = tone = Farbenton, (2) brightness = luminosity = Helligkeit, (3) saturation = purity = Sättigung. Hereinafter we shall name them hue, sensation-brightness and saturation. The unqualified word 'brightness' is confusing; for it is used by Mr Smith to denote a stimulus in some sentences, a sensation in others. We also have formerly not been sufficiently careful in this matter. The word 'tone' is ambiguous, for artists use it to mean degree of brightness or darkness, and so we shall avoid it. This classification is not quite universal, for Myers[2] uses four qualities: hue, intensity, saturation and brightness. However, we shall here follow the majority in supposing that three independent qualities are enough.

Pink, as popularly understood, differs from scarlet in being (1) a brighter sensation, (2) less saturated. It may or may not differ slightly, (3) in hue.

This statement can be made clearer by reference to water-pigments spread on white paper. On looking at a mixture of Chinese white with Messrs Winsor and Newton's alizarin scarlet, we experience a sensation that is brighter and less saturated than, and almost of the same hue as, that produced by the alizarin scarlet alone in a thick coat. The former sensation would ordinarily be called pink.

It is possible to produce a sensation which differs only in saturation from that due to a thick coat of alizarin scarlet laid on white paper. The appropriate stimulus can be arranged by adjusting a mixture of the same scarlet with Indian ink and burnt umber. But the result is not pink in

[1] Sir John Herbert Parsons, *An Introduction to the Study of Colour Vision*, p. 32, Camb. Univ. Press, 1924.

[2] C. S. Myers, *A Text Book of Experimental Psychology*, I, 71, Camb. Univ. Press, 1925.

any ordinary sense. It would have to be called a pinkish brown. The cover of this *Journal* is an example of pinkish brown; the cover of Parsons' *Colour-Vision* is another, but less bright and more saturated.

"Our feeling of pink is not a portion of our feeling of scarlet" thus refers to sensations which differ in saturation, as well as in brightness. In testing the truth of this dictum, both sensation-brightness and saturation must be varied; they must be varied together, not independently; and the observer must be asked to make a judgment on the effect of sensation-brightness and saturation jointly. We asked the observers to judge on a scale of 'redness'; for in framing instructions popular words had to be used.

It may be said that such a procedure is a muddle; for it would be more scientific to try quantitative mental estimates of sensations when hue, sensation-brightness and saturation were varied separately one at a time. We agree, and hope that such experiments will be made. But they would not test James' dictum. That is why we deferred them.

A possible defence of James not mentioned by Mr Smith, would be to point out that we have replaced James' doublet 'pink and scarlet' by a triplet 'white, pink and scarlet.' To use a geometrical phraseology we have dealt with sensation-intervals; James may possibly have intended sensation-points.

It is generally agreed that formless visual sensations are best classified by arranging their representative points in a volume. For example, C. S. Myers[1] gave a diagram in the shape of two cones, base to base, with white at one vertex, black at the other vertex, the greys along the axis. So that sensation-brightness increases steadily along the axis, saturation increases with distance from the axis and hue depends on angular position round about the axis.

We now[2] add the further, and controversial, idea that the distances of the representative points can be so chosen that when point $B$ lies on the straight line $AC$, the ratio of lengths $AB : BC$ becomes equal to the ratio of the corresponding sensation-intervals as judged by an average observer.

From the way in which the pink stimulus was made on our colour wheels, it was evident that as the energy in the light-waves increased, owing to increasing angle of the white sector, so also the distribution of that energy would spread into wave-lengths shorter than 0·6 micron.

[1] C. S. Myers, *An Introduction to Experimental Psychology*, p. 18, Camb. Univ. Press, 1911.

[2] Apparently in agreement with Peddie, *Nature*, p. 791, Nov. 23rd, 1929.

Sensationally it appeared to our eyes that the pink was intermediate between the white and the scarlet, both in saturation and in sensation-brightness. We purposely arranged it so. Thus we think that the white, pink and scarlet as exhibited by our colour wheels lay in an axial plane of Myers' cones, and in a straight line in that plane.

Let the representative points be $A$ for white, $B$ for pink, $C$ for scarlet. Let their normal projections on to the axis of the cones be $A_1, B_1, C_1$, and their projections on to the basal plane be $A_2, B_2, C_2$. Then because $ABC$ is straight, therefore $AB : BC = A_1B_1 : B_1C_1 = A_2B_2 = B_2C_2$. If the observer judged by sensation-brightness alone (as Mr Smith alleges), he would estimate $A_1B_1 : B_1C_1$. If he judged by saturation alone, as the instruction about 'redness' may perhaps have suggested to some observers, he would estimate $A_2B_2 : B_2C_2$. If he judged his sensations without troubling to analyse them into saturation and brightness, he would estimate $AB : BC$. But all these ratios are equal. This may explain how Mr Smith's formula, derived from considerations about sensation-brightness, comes to be such an excellent fit to observations involving saturation.

Was the influence of the experimenter in our observations misdirected or excessive? We think that it was neither. In Richardson's research some eminent observers were quite willing to try the experiment; and the mean of their estimates (see Group 8) for the wheel was 44 per cent., in close agreement with the mean of 43 for all the men, and not far from the mean of 40 for all the schoolboys in Maxwell's research.

Mr Smith remarks, "their fundamental question was put in such a way as to ensure an answer apparently unfavourable to William James." The word 'ensure' is an exaggeration, for observers were at liberty to refuse to put any mark on the line, and the numbers of those who did so refuse are published. Mr Smith might truly have said 'suggest' instead of 'ensure.' But as the task of estimation requires some effort of attention, it is necessary to persuade the observer to try.

In spite of Mr Smith's criticisms, we continue to think that, by attacking James' dictum about pink and scarlet, we have cleared the way towards a more scientific study of quantitativeness in sensations of various kinds. The rare mistakes of a genius like James are apt to enslave the rest of us.

[1] Richardson, "Quantitative Mental Estimates of Light and Colour," this *Journal*, xx, 1, 27–37.

(*Manuscript received* 30 *December*, 1929.)

# THE AVERAGE VALUE FOR THE PROBABLE ERROR OF TETRAD DIFFERENCES.

By C. SPEARMAN and K. HOLZINGER.

In 1925 an average value* for the probable error of all the tetrad differences in a table was given. This value, while good enough for most practical purposes, may be slightly improved by considering certain omitted products. The new formula was given as 16 A in the Appendix of *Abilities of Man*† but was derived too late for a proof to be included in the text. It is the purpose of the present paper to furnish the proof of the new approximative formula.

To obtain the new value we require the average of

$$N\sigma_t^2 = (r_{12}^2 + r_{13}^2 + r_{24}^2 + r_{34}^2) - 2\,(r_{12}r_{13}r_{23} + r_{12}r_{14}r_{24} + r_{13}r_{14}r_{34} + r_{23}r_{24}r_{34}) + 4r_{12}r_{13}r_{24}r_{34} \quad \ldots\ldots(1).$$

I. Consider first the terms in $r^2$. If $\bar{r}$ denotes the mean of the correlations in the table, $s^2$ their variance and $n$ the number of variables, the mean of the different terms in $r^2$ is $\bar{r}^2 + s^2$.

Therefore

$$\sum_1^{3C_4^n} (r_{12}^2 + r_{13}^2 + r_{24}^2 + r_{34}^2) \div 3C_4^n = 4\bar{r}^2 + 4s^2 \quad \ldots\ldots(2).$$

II. Consider next the terms in *rrr*. For the whole table there are $3C_4^n$ sets of terms like $(r_{12}r_{13}r_{23} + r_{12}r_{14}r_{24} + r_{13}r_{14}r_{34} + r_{23}r_{24}r_{34})$ or a total of $12C_4^n$ terms in *rrr*‡. The number of different terms of the form $r_{12}r_{13}r_{23}$ is $C_3^n$ since it is equal to the number of ways in which three subscripts may be selected from $n$. The number of repetitions of

$$\sum_1^{C_3^n} r_{12}r_{13}r_{23} \text{ is therefore } 3\,(n-3).$$

Therefore

$$-2\sum_1^{3C_4^n} (r_{12}r_{13}r_{23} + r_{12}r_{14}r_{24} + r_{13}r_{14}r_{34} + r_{23}r_{24}r_{34}) \div 3C_4^n = -\frac{48}{n\,(n-1)\,(n-2)} \sum_1^{C_3^n} r_{12}r_{13}r_{23} \quad \ldots\ldots(3).$$

* This *Journal* (2), XVI, Oct. 1925, 86. The same value is obtained by Pearson in *Biometrika*, 1927, XIX, 257.

† C. Spearman, Macmillan & Co., Ltd., London, 1927.

‡ Really there are $6C_4^n$ sets of terms. But as in every case the square of $r_{13}r_{24} - r_{23}r_{14}$ must equal that of $r_{23}r_{14} - r_{13}r_{24}$, we need only deal with $3C_4^n$ sets.

Next expanding $r_{12}r_{13}r_{23}$ we find

$$(\bar{r}+d_{12})(\bar{r}+d_{13})(\bar{r}+d_{23}) = \bar{r}^3 + \bar{r}(d_{12}d_{13}+d_{12}d_{23}+d_{13}d_{23}) + \text{neglected terms.}$$

The number of different products like $d_{12}d_{13}$ is $3C_3^n$. Therefore

$$\sum_1^{C_3^n} r_{12}r_{13}r_{23} = C_3^n\bar{r}^3 + 3C_3^n\bar{r}\,\overline{d_{12}d_{13}} \qquad \ldots\ldots(4),$$

where the bar indicates mean value.

The value of $\overline{d_{12}d_{13}}$ may be approximated closely by noting that

$$\overline{d_{12}d_{13}} = \overline{d_t^2} \qquad \ldots\ldots(5),$$

where $d_t$ denotes the deviation of the mean of a tetrad from $\bar{r}$. Also $16d_t^2 = (d_{12}+d_{13}+d_{24}+d_{34})^2$, whence,

$$\overline{d_t^2} = \tfrac{1}{4}s^2 + \tfrac{1}{2}\overline{d_{12}d_{13}} + \tfrac{1}{4}\overline{d_{12}d_{34}} \qquad \ldots\ldots(6).$$

The total different terms of type $d_{12}d_{13}$ for $n$ variables is $3C_3^n$, the total terms of type $d_{12}d_{34}$ is $3C_4^n$, and the total terms of both types is $C_2^{C_2^n}$. Using Yule's* theorem we have

$$\text{Mean of both types} = -C_2^{C_2^n}\frac{2}{(n-1)(n-2)}s^2 = 3C_3^n\overline{d_{12}d_{13}} + 3C_4^n\overline{d_{12}d_{34}}$$

and

$$\overline{d_{12}d_{34}} = \frac{-2s^2}{(n-2)(n-3)} - \frac{4}{(n-3)}\overline{d_{12}d_{13}} \qquad \ldots\ldots(7).$$

Combining (5), (6) and (7) we obtain

$$\overline{d_{12}d_{13}}\dagger = \frac{n-4}{2(n-2)}s^2 \qquad \ldots\ldots(8)$$

and

$$\overline{d_{12}d_{34}} = \frac{-2s^2}{(n-2)} \qquad \ldots\ldots(9).$$

Substituting the value of (8) in (4) and then in (3) gives

$$-2\sum_1^{3C_4^n}(r_{12}r_{13}r_{23}+r_{12}r_{14}r_{24}+r_{13}r_{14}r_{34}+r_{23}r_{24}r_{34}) \div 3C_4^n = -8\bar{r}^3 - 12\bar{r}\frac{(n-4)}{(n-2)}s^2 \ldots\ldots(10).$$

III. Consider next the terms in *rrrr*. We may write

$$(\bar{r}+d_{12})(\bar{r}+d_{13})(\bar{r}+d_{24})(\bar{r}+d_{34}) = \bar{r}^4 + \bar{r}^2(d_{12}d_{13}+d_{12}d_{24}+d_{12}d_{34}+d_{13}d_{24}+d_{13}d_{34}+d_{24}d_{34}) + \text{neglected terms.}$$

* Yule, *Introduction to Statistics*, chap. XI, § 11.

† An *exact* value of $\overline{d_{12}d_{13}}$ is $\frac{\Sigma(\Sigma' d_{xy})^2}{n(n-1)(n-2)} - \frac{s^2}{n-2}$, where $\Sigma'$ is the summation of $d$'s with a variable in common. Also $\overline{d_{12}d_{34}} = \frac{2s^2}{(n-2)(n-3)} - \frac{4\Sigma(\Sigma' d_{xy})^2}{n(n-1)(n-2)(n-3)}$.

For the whole table terms like $d_{12}d_{13}$ are repeated $(n-3)$ times and terms like $d_{12}d_{34}$ are repeated twice. Therefore

$$4\sum_{1}^{3C_4{}^n} r_{12}r_{13}r_{24}r_{34} \div 3C_4{}^n = 4\bar{r}^4 + 4\bar{r}^2\,[2\overline{d_{12}d_{34}} + 4\overline{d_{12}d_{13}}]$$

$$= 4\left[\frac{2\,(n-6)}{n-2}\,\bar{r}^2 s^2 + \bar{r}^4\right] \quad \ldots(11),$$

upon utilizing equations (8) and (9).

Combining the results of equations (2), (10) and (11) gives finally

$$\overline{N\sigma_t{}^2} = 4\left\{\bar{r}^2\,(1-\bar{r})^2 + \left[1 - 3\bar{r}\,\frac{(n-4)}{(n-2)} + 2\bar{r}^2\,\frac{(n-6)}{(n-2)}\right] s^2\right\} \quad \ldots\ldots(12)$$

or $$\overline{\text{p.e.}_t} = \frac{1{\cdot}349}{\sqrt{N}}\sqrt{\bar{r}^2\,(1-\bar{r})^2 + \left[1 - 3\bar{r}\,\frac{(n-4)}{(n-2)} + 2\bar{r}^2\,\frac{(n-6)}{(n-2)}\right] s^2}$$

$\ldots\ldots(13)$*.

Using the *exact* values of $\overline{d_{12}d_{13}}$ and $\overline{d_{12}d_{34}}$ of the footnote on p. 369 gives

$$\overline{\text{p.e.}_t} = \frac{1{\cdot}349}{\sqrt{N}}\sqrt{\left\{\bar{r}^2\,(1-\bar{r})^2 + \left[1 + \frac{6\bar{r}}{n-2} - \frac{4\bar{r}^2\,(n-4)}{(n-2)\,(n-3)}\right] s^2 - 2\,\frac{\Sigma\,(\Sigma' d_{xy})^2}{n\,(n-1)\,(n-2)\,(n-3)}\,[3\,(n-3)\,\bar{r} - 2\,(n-5)\,\bar{r}^2]\right\}}$$

$\ldots\ldots(14)$.

This last value is too clumsy for ordinary use and by empirical test gives values often no better than (13) because of the neglected terms in expansions of *rrr* and *rrrr*. We therefore recommend the use of (13).

* This is the formula given as 16 A in the *Abilities of Man* and employed in that work.

(*Manuscript received* 19 *November*, 1929.)

# A SUGGESTED TERM:—EUPHASIA.

By T. H. PEAR.

(*From the Department of Psychology, University of Manchester.*)

In two recent publications[1] I have tried to separate skill-in-general, defined as the integration of well-adapted responses, from ability for deliberate, adequate, verbal expression. This latter ability, from one point of view, is often a special type of skill[2]. Yet nowadays it is such an important type—indeed, perhaps unique, in that it can often serve as a substitute for most of the others, as in giving orders to skilled people—that it merits a special name.

I have explained at length why 'speech-habits' seems a highly unsuitable term. The proposed use of the word 'intellect' for this ability has aroused no enthusiasm, and the meanings attached to intellect seem hopelessly irreconcilable. Yet it is urgently necessary to distinguish skilled persons who cannot verbally describe their skill from skilled persons who can; and, not less importantly, the cultured persons who, though they cannot perform skilled actions in the arts, both fine and utilitarian, can describe and criticise some of them effectively.

On the need being explained to Prof. W. B. Anderson of the Latin Department of the University of Manchester, he has suggested 'euphasia' to indicate the ability for deliberate, adequate verbal expression. Since aphasia and dysphasia, with their useful adjectives, have justified their inclusion in the vocabulary of science, there seems little need to urge in detail the international claims of 'euphasia.' In the hope that it may help to decrease the agglutination of psychological concepts involved in the use of the word 'habit,' and assist in analysing the complex of processes denoted at present by 'verbalization,' it is here proposed, with grateful acknowledgments to Prof. Anderson.

[1] *Fitness for Work*, pp. 55 ff., London 1928, and "Some subtler skills," this *Journal*, xx, 1929, 146 ff.

[2] Cf. McGeoch, "The acquisition of skill," *Psychological Bulletin*, xxvi (8), 457, August 1929; and Gemelli. "Recherches sur la nature de l'habilité manuelle," *Jour. de Psychologie normale et pathologique*, 1929, 3 & 4.

(*Manuscript received* 19 *February*, 1930.)

# THE FUNCTIONS OF THE CRITIC.

By SUSAN MILES.

Is the critic, we may ask ourselves, a teacher? Is he a touchstone? Is he either, neither, or both? He is, surely, other than the teacher, and more than the touchstone.

A teacher may show an artist how to drive his vehicle up to his medium; he then withdraws. It is Haydon's word: 'vehicle.' He tells us that in his early days he found his colours drying too quickly. He appealed to an older painter for advice. "What d'ye paint in?" asked the older painter. "Drying oil and varnish." He was told to use raw oil; and he had to ask what raw oil was. He was delighted with the information that it was oil not boiled; and he used raw oil thenceforward. "Such was at that time," he tells us, "my ignorance of vehicle."

The function of a vehicle is to save a person's time, vitality, and temper. It is an important function, and an artist's time, vitality, and temper are particularly well worth saving. A teacher may save an artist's time, vitality, and temper in the same way that Mrs Beeton may save the housewife's.

The distinction between vehicle and medium, or, better, between driving the vehicle and using the medium, needs to be made clearer. Perhaps the point may be put in this way: artists and other people too may be taught to drive a vehicle—and the same vehicle. The knowledge that raw oil keeps its moisture longer than boiled oil is, presumably, useful to the house-decorator as well as to the painter of pictures. But when the painter of pictures has got out of his vehicle, and is standing on his own feet up against his medium, he begins to be concerned with something with which the house-decorator has no concern. The artist is concerned now with his technique. His teacher can teach him how to prevent his colours from drying too quickly, but not what colours to use, or what shapes to make, in order to express his own peculiar and individual aesthetic impressions. Again, poets and other people too can be taught the normal use of language: the names of things, the structure of sentences, and so on. The poet may travel in the same cart that carries the huckster to market. The exciting time begins when he gets out of the

cart and begins to tackle his own job. And it is when the poet has got down from his cart that the critic's function may begin to be exercised.

Not, of course, that there is necessarily any temporal break between the artist's concern with vehicle and his concern with medium. He may go on learning such facts as that about boiled oil long after he has begun to learn his own technique. The use of the 'vehicle' metaphor can easily be pushed too far. It might be better to make the distinction by the use of other terms, and to speak of public, as distinct from private, technique. The fact that, in practice, it may often be difficult to distinguish between driving the vehicle and using the medium (or between public technique and private technique), does not mean that the difference is unimportant from the point of view of theoretical analysis.

Neither kind of technique is the concern of the touchstone. He may be ignorant alike of vehicle and of medium. Yet, in his different way, he may be of as much use to the artist as the teacher—or more. The touchstone's function is to register his own reactions to a work of art. A nod, a shake of the head, a blank look, a raising of the brows, may be register enough in the case of some touchstones in relation to some works of art. "Mary is staring dully into the fire: this has gone flat." "Mary's eyes shine: this has come off."

Perhaps the point at issue for the artist is: "Does it matter that the dates don't fit? Or that the physiology, or the topography isn't accurate?" For the artist himself the intensity of passion pushes all inconsistencies on to another plane, so that the figures of his creation speed by unimpeded to their goal. But will these inconsistencies seem to his touchstone the mere irrelevancies that they seem to him? Better still, will he be swept on with the characters without so much as realizing that inconsistencies or inaccuracies are there? Will the touchstone be convinced, on the plane of art, by what, on the plane of fact, would be impossible?

The author as he reads aloud watches apprehensively. Will that little furrow in the brow display itself, the little pout of the lower lip, the derisive gleam in the eye? Or will there come instead the quick flush, the tremor of the sensitive lip, the sudden upthrust of the chin—and triumph?

The ideal touchstone is hard to find, and when found more precious than rubies. Some of his—more probably of her—characteristics may be enumerated. She must possess utter sincerity. A single smile of polite and meaningless affability will cut away all confidence. She must be disinterested enough to make a complete fool of herself if needs be without a quiver of hurt pride. There must be the minimum of emotional inhibi-

tions dependent upon personal associations of which she is unaware. No muddied depths must mar the crystal of her clear-mirroring pools. That she must have sensitiveness of response goes without saying: that "trembling, delicate, snail-horn perception of beauty," as Keats has called it. But as well as sensitiveness of response there must be stability. Better stony ground and prickly thistles than a quicksand.

The pure touchstone, it must be noted, makes no attempt to help the artist mend his ways. She leaves him merely with his convictions ratified or shaken. Something is wrong, or something is right; now it is the artist's business to set the wrong right, if wrong it is: the artist's own business—and the critic's; but not the touchstone's.

Before attempting to define the functions of the critic proper, it may be worth while to say something about them which probably no one would dispute. The function of the critic is dependent upon and secondary to that of the artist. This is obvious. But though it is obvious, it has some important implications which are not so obvious. One implication is that to discuss the critic's function without having first come to some conclusion as to the artist's function would be futile. Anyone who writes explicitly about criticism must write implicitly about art. Another implication is that a profitable discussion as to the nature of criticism can take place only among people who have had artistic experience. It is a first-hand appreciation of works of art that is essential, not a wide knowledge of art-criticism. We cannot deduce the nature of criticism from the study of the work of critics. But we can, or we ought to be able to, deduce the nature of criticism from our own experience—not of criticism—but of art.

It will be well at this point to summarize in the briefest possible phrases the conception of art that underlies the conception of criticism about to be set forth. Art is the adequate expression, in a medium, of an impression which the artist has experienced as worth while in itself. If this definition of art is accepted it will be possible to define the functions of the critic in derivative terms, and to suggest that the critic's functions are, first, to decide whether or not that which purports to be a work of art is an attempted expression of an aesthetic impression, and, under certain circumstances, to describe and analyse the reactions on which his decision is based; and, second, to decide whether or not the expression, if it *has* been attempted, is adequate; and, under certain circumstances, to expound either to the artist, or to the artist's audience, the nature of the artist's success or failure.

The critic must first of all determine whether or not that which

purports to be a work of art *is* an attempted work of art. Is it based on something genuine or is it spurious? Has the person purporting to have produced a work of art ever had the aesthetic impression which he purports to have expressed? If he has not, there is no work of art under consideration; and it may well be that the critic's function in relation to the counterfeit is at an end. It may—or it may not—be worth while to let the pseudo-artist know that his pretensions have been noticed; it may—or it may not—be worth while to warn an existing audience that it has been duped, or a potential audience that it may be duped. Such notifications and warnings may be classed under the heading of criticism; they may indeed be a very useful part of the critic's output. They may be useful, that is to say, to the pseudo-artist or to his public, assuming—but it is a very big assumption—that the pseudo-artist wants to be a real artist, or his public to find one. But for the critic himself there is danger ahead. The study of pseudo-art is not a good preparation for, nor a good form of relaxation from, the study of genuine art. Probably not even the greatest of critics could read, say, fifty bad novels a year without being the worse judge of a Hardy at the end of it. Each bad novel will deposit its modicum of impurity to sully the clear crystal of the touchstone. And the critic, though more than a touchstone, must be a touchstone. He cannot describe and analyse his reactions unless he has reacted.

It is the critic's first business, then, to test his reactions and to decide whether or not they are genuinely critical or merely emotional. This testing may take much time and much trouble. It may involve the repeated examination of his object on different occasions and in varying moods. It may involve the study of other works of art which may in their turn act as touchstones. A critic may be carried away by, shall we say, Miss Kaye Smith's *Green Apple Harvest*, and then may re-read *The Woodlanders* to test his first reaction. Or, again, he may test his own immediate reaction by the application to it of general principles of criticism which have been evolved, either by himself or by other critics, from the study of other works of art. This may be a very valuable part of the critic's activity. Yet it too may be perilous. It will be perilous to any critic who fails to realize that all sound critical dicta will be applicable to the uncreated masterpieces of the future, as well as to those which already exist. The applicability of a critic's canons to works of art uncreated in the critic's lifetime affords the best of all possible testimonies to that critic's worth. Failure to fit in with a canon can never be the ultimate test of a work of art. In that it does not fit may lurk the condemnation, not of the poem or of the picture, but of the canon. The

canons of art were not "fixed long ago by certain inspired writers whose authority it is no longer lawful to call in question," as was once asserted by an early contributor to the *Edinburgh Review*. They have been, and they are still being, deduced from a patient study of the works of art themselves. If something comes into being which is recognized intuitively as a work of art, and which continues so to be recognized after repeated tests, then any canon of art which is irreconcilable to it must be modified. The critic, even more than anyone else, must learn to expect the unexpected.

Supposing for instance that Hardy's *Dynasts* may justifiably be classed as an epic, then, since it is intuitively recognized as a work of art, Aristotle's dictum that an epic should be written in heroic metre throughout needs reconsideration. Since the critic's function is secondary to, and dependent upon, the artist's, it would be absurd to say: "Aristotle was a great critic, therefore *The Dynasts* cannot be a great epic." It is proper to say: "*The Dynasts* is a great epic, therefore Aristotle, though a great critic, erred in maintaining that an epic must be written in heroic metre throughout." But facts have never given the lie—nor are they likely ever to give the lie—to his assertion that every work of art should have a beginning, a middle, and an end. We shall do well to apply the one canon while refusing to apply the other.

When a work of pseudo-art is so much like a genuine work of art as to deceive even careful students, it may be very well worth while for a critic to spend time and trouble in analysing his own reactions and helping other people to see through the deception. When the spurious quality of the work is obvious, to spend time and trouble upon it is undesirable; the critic's time had clearly better be spent on something else.

Let us now pass on to consider the critic's second function, and consider him no longer as concerned in determining whether or not there has been any aesthetic impression to express, but in determining rather whether or not the expression of a genuine aesthetic impression is adequate, and in what respects it is adequate or inadequate. We now find ourselves faced by a very difficult question. Presumably the critic is aware of the artist's impression only through his attempt at expression; but if the attempt at expression has failed, how does the critic know what it was that the critic has failed to express? What is it that he puts over against the work of art with which to compare it?

There is an answer which at first sight, but at first sight only, may appear to be adequate. The expression, it is urged, has only partially failed. In some ways it must have succeeded, or else the critic would not

be able to assert that the artist's aesthetic impression had taken place. The work of art is inadequate in the same way in which a torso is inadequate. Bits of the whole are lacking, but enough are there to enable us to infer the missing bits. It is merely a case, it is urged, of Prof. Stout's 'Relative Suggestion,' or of Prof. Spearman's 'Eduction of Correlates.' It is a case of filling up the gaps in a discontinuous series. There are enough dots there for the critic to be able to fill in the dots that are not there but that would have been there had the artist been successful.

The first part of this argument is certainly sound. It is true that the expression must have partially succeeded. What is unsatisfactory in the proffered solution is that, if there are enough dots there to enable the spectator to fill in the rest and so get his continuous series, then the attempt at expression has not failed. The work of art is adequate. "It is more blessed to guess than to be told." If there had been more dots and less guessing the work of art would have been less, and not more, successful.

No, we are still faced by the very difficult question: how does the critic know what the artist was trying to express but has not expressed? How does the critic get into touch with the artist's aesthetic impression otherwise than through the artist's expression? I think that we are driven to the conclusion that the critic is himself an artist, and that by some blessed coincidence he has himself experienced an aesthetic impression similar enough to that experienced by the other artist for him to know that it ought to have been expressed thus, or thus. (It is important to note that we are considering the case of the critic faced by an inadequate work of art, not of the mere spectator appreciating an adequate one. The two situations are very different.)

Two objections may be raised to the conclusion that the constructive critic is an artist. Great artists, it may be urged, often make futile criticisms of other artists' work, and valuable criticism is often given by critics who have themselves produced no great works of art.

These statements are true in themselves, yet they do not disprove the conclusion against which they are aimed. They may be met by the consideration that most great artists are too much occupied with their own creations to spare time for constructive criticism of other people. If pressed to give criticism when they are itching to do their own work, they may, either out of deliberate perversity, or as a defence mechanism, say some very foolish or some very spiteful things. Even when they want to be useful, and fair, and constructive, they may be so much obsessed by their own creations that they cannot see round them. They squint, or

have blind spots. Probably every great artist would be a great critic if he were not obsessed by his own creations; but every great artist is apt to be obsessed by his own creations—except when he is exhausted and so unfit to give constructive criticism. Hence the extreme rarity of good constructive critics! Hence too the unsystematic nature of the best criticism. It seems to be wrung out of the great artists half against their will. Or, when it is given ungrudgingly (as, say, by Charles Lamb), it leaves one with the impression that the critic might have been an even greater artist than he was if he had been a less acute and subtle critic. Lamb was essentially a generous man. Had he been less generous, might he have been as great a poet as Wordsworth? Had Wordsworth been less grudging, less egocentric, might he have been as great a critic as Lamb? After all, was it not Wordsworth who said of Blake, at a time when Blake's genius was barely recognized: "There is something in the madness of this man that interests me more than the sanity of Scott?" Wordsworth did not want to read other men's books. He feared them perhaps. If Wordsworth had kindled to Shakespeare and Marlowe as Lamb did, would he have written about them instead of writing *Tintern Abbey*? We cannot tell; the case is, in the nature of things, incapable of proof.

But the difficulty of finding any other satisfactory solution of the question we have been considering seems to afford a strong presupposition for the belief that the greater the artist the greater the critic—provided he puts himself wholeheartedly into his criticism. It is, however, only under exceptional circumstances that the great artist will put himself into the work of criticizing. Of criticizing other people's work, that is to say. He is, of course, continually criticizing his own: a fact which is often lost sight of, but which goes some way towards the support of the contention here put forward. The truth may, perhaps, be expressed in this way: all great artists would be great critics if they gave their minds to it, and all great critics are, in flashes, great artists.

The critic and the artist have, perhaps, something in common with the two dogs of Mr Liam O'Flaherty's study. The greyhound was some years younger than the black dog. His coat was glossy as silk, and his snout was so long and pointed that he could lick milk from the bottom of a tumbler without touching its sides. Almost without moving a muscle he could jump from a standing position and go clean over his master's head without grazing it. But while the black dog methodically nosed along a rabbit's trail, the greyhound rushed about furiously, jumping stone walls with his head in the air. When the black dog started a rabbit,

the greyhound outdistanced him in a few strides, but was unable to catch the rabbit because of his own terrific speed, and the rocky ground which cut his delicate paws and made him stumble. And often the greyhound turned a rabbit into the black dog's mouth; but he seldom caught a rabbit for himself.

Black dog and greyhound: both chase rabbits. Poet and critic: both pursue the same elusive prey. Greyhound critic with his sudden swift onrush speeds past blackdog artist, and turns the rabbit beauty into black dog's jaws. O'Flaherty's black dog did not love the greyhound, although the greyhound's speed and delicacy would sometimes help him catch a rabbit he would otherwise have lost. Nor does the artist always love the critic, even when the critic offers him sound and constructive criticism.

And is the critic the artist's foe? The trend of our argument has raised the question. If the critic is an artist who makes criticisms instead of art, is not criticism art's enemy? The greyhound, if he kept off the black dog's preserves, would start his own rabbits, and learn to catch them? Perhaps. And perhaps in an ideal universe there would be no place for criticism, however constructive. Art and art appreciation would form a whole completely rounded. Perhaps it is a good thing, even now, to work for the elimination of the critic and the independence of the audience. (For the independence of the artist there is little need to work—because of the natural intransigence of the creature.) Criticism is not, perhaps, of surpassingly great intrinsic value. And yet—as things are, in an unideal universe—we may be thankful for the criticism that is wrung from the artists. Probably the less professional criticism—as a thing apart, put forward by the critic who does not himself create—the better.

But the question has become that of the antagonism, if any, between art and criticism. To get back to the concrete. Is there a natural enmity, not between art and criticism, but between the artist and the critic? It would not be surprising to find such enmity; and that not primarily because of the self-centredness so common to our kind—"a poor thing, but mine own" seeming so often better than a good thing but another's—for in great art we do, surely, get beyond mere personal possessiveness. We would, probably, save our own child, even though he had an intelligence quotient of forty, if it were a choice between saving him and drowning Plato. But would Keats have saved *Endymion* at the expense of *King Lear*? It is hard to believe it. And even if he could not have brought himself to cast away his first-born, it would not have been the artist in

him that flinched, but the mother—so often there along with the artist, but other than and often in conflict with him—the mother in Keats, who couldn't bear that this youngster should perish.

No, it is not mere self-centredness on which the enmity is based, but upon fear. Fear lest the great artist that the great critic is should swallow up the great artist that the artist *manqué* might be. No two men can have identical impressions; hence, however right the critic's contribution may be as an expression of the impression he has had, yet it cannot be quite right as an expression of the original artist's impression. The critic's contribution is towards a new work of art, greater than any that the fumbling artist could, at that time, have made by himself, but—however slightly—different in kind from the work of art that would have been made if the first artist could have made one by himself. And it may well be this fact that explains the stubbornness of young artists in the face of even the most generous constructive criticism.

Assuming then that we are justified in supposing that a critic is a constructive critic only in so far as he is an artist—albeit an artist fleetingly and fragmentarily—the wonder of the critic's faculty of seizing upon the artist's unexpressed impression merges in the greater wonder of the artist's faculty itself. With that, explicitly, we are not here concerned.

It is time now that we touched upon the critic's lesser functions in relation to the artist's audience: for the critic has much to do in addition to his work in relation to the artist himself. These lesser functions do not in themselves need an artist to carry them out. At its simplest, the critic's function may be little more than a gesture. He is the finger pointing; the voice that cries: "Look!" This is the anthologist's task; yet even the anthologist is a critic, since the very act of singling out that at which to point involves a critical discrimination. The editor too may be a critic, and that apart from any commentary. He too is choosing, and by his act asserting the value of that towards which he directs attention.

Nor must we fail to mention the function of the parodist, for parody, as Sir Owen Seaman has told us—and shown us—is a department of pure criticism. Not, of course, that form of parody which is the outcome of mere high spirits; there is no room for facetiousness in critical parody such as that of Mr Max Beerbohm or of J. K. Stephen. The work of the critical parodist is twofold: he may make a genuine artist realize what might happen to his work if he relaxed his own criticism; and he may show up the mannerisms, affectations, and pretensions of a pseudo-artist

with a reputation. In some cases criticism can take no better form than parody: above all in the case of platitude masquerading as profundity, or sentimentality as pathos. Parody at its best may be described as condensed criticism; a really good parody may concentrate into half a dozen lines a critic's judgment, not only as to a writer's profundity—or his lack of it—but also as to his psychology, his phrasing, and his rhythms.

It may be objected that such criticism is necessarily destructive. That is true. But although destructive, such criticism is by no means necessarily condemnatory. Indeed, to be subtly parodied by a good critic is in itself a commendation. No good critic is likely to spend much time in parodying either a consistently bad or a mediocre writer. For to parody is not a quick and easy form of criticism. It takes pains as well as skill to catch the phrasing and the rhythm and the substance, and to set them free again with just those additions that will make them absurd without making them unrecognizable.

A few artists have been detached enough to be able to parody themselves—deliberately. It is as much as we can expect from most if they can succeed in deriving profit and amusement from other people's parodies of their work. As a matter of fact few artists are as detached as Henley, who is said to have remarked that he might have written Sir Owen Seaman's "Out of the large limbed night" himself—when he was drunk. The qualification, by the way, is significant, though it would have been better if Henley had said "after a few glasses of champagne" rather than "drunk": just enough to break down the barriers of self-criticism. But champagne there must be. To produce an imitation that might have been an original written after a too heavy high tea is no achievement. That Scott included Goldsmith's parody of Swift in his edition of Swift's works under the impression that Swift wrote it is no less a criticism of Goldsmith than of Scott.

Closely allied to parody is the mock panegyric, such as Edward Thomas' ironic eulogy of Ella Wheeler Wilcox. Again, there is the criticism that lies behind the interweaving of quotations such as Mr Leonard Woolf's deft mingling of Mr Middleton Murry's phrases with those of Mr Pecksniff. And there is Mr Desmond Macarthy's neat juxtaposition of Miss Stein's aphasic sentences and morsels from a typist's book of exercises.

Criticism of this type may be described as demonstrative criticism. Mr Christopher Stone, in his little book in the Art and Craft of Letters' series, alludes to it more vividly as criticism of the "Sister Mary walks like this" type. It needs to be done well to be tolerable, but when it is

done well it is well worth the difficult doing. Andrew Lang, for instance, in his *Brahma*, has contributed a genuine piece of criticism, not only of Emerson's method and phrasing, but of the whole school of that idealistic philosophy which delights in "setting things up over against one another and bidding them proclaim their mutual identity."

> I am the batsman, and the bat;
> I am the bowler, and the ball,
> The umpire, the pavilion cat,
> The roller, pitch, and stumps, and all.

It is tempting to quote more examples of such criticism, but, instead, let us turn for a few moments in conclusion to the consideration of some suggestions thrown out by the late Katherine Mansfield. "Style," she once said, "is speaking to the back of the room." The saying shows her customary insight. Style is just that: being audible without straining either your voice or your hearer's ears, and without shouting. And the critic, who must himself have good hearing, is the man who sits in the back row, and determines, if the speaker is not heard, whether it is because he is dropping his voice or because the audience is deaf. Note that it is not the critic's business to be a megaphone. "Sa fonction est de dire la vérité, mais non pas de la faire croire," as Rousseau said. It is the propagandist, not the critic, who hammers in his own or other people's points. But the critic may well put forward a guess of his own, when the speaker has finished, to fill in a gap in a good speech which has been mumbled here and there. And he may well throw out a suggestion as to the choice of a building. In the Albert Hall a speaker *must* strain his voice and do outrage to the senses of the people in the front rows—unless he is to be inaudible in the gallery. It is part of the critic's function to point out this fact to anyone whose voice has qualities worth saving. To point out the fact and, perhaps, to help him find a building that will give the right range for his voice: the Queen's Hall, it may be, or the Poetry Bookshop—or a powder closet. Sometimes it may be the critic's business to invite a few friends to his own study and beckon the speaker to talk to them there.

In such terms as these might Katherine Mansfield have extended her metaphor to include not only style but the criticism of style. Instead she made another suggestion. "How," she asked, "is one to test the range of one's voice?" and her answer was that one should find a very simple friend and experiment upon him. Yes, Katherine Mansfield's friend is, perhaps, what has been called the touchstone. But there is a problem here. "How simple is the friend to be?" And the best answer seems: "He should be just as simple as the artist himself, but no simpler."

Indeed, the ideal critic of an artist's work is the artist himself stripped imaginatively of all prior knowledge of what he is trying to express. The artist's style must be grounded in utter honesty. He must speak to an audience no more acute and no less acute than himself. His test must be: "If I had been in the audience should I have needed that gesture, that inflection, that emphasis?" If not, it must go. "Should I have followed that ellipsis, have supplied the implications of that phrase?" If not, something is lacking and must be added. It is a question of how many dots would the artist himself have needed to find the continuity of the series and make the curve. The difficulty is, of course, for the artist to strip himself in imagination of that which is there in fact. His knowledge *is* there; it is hard for him to get away from it. Hence the value to the artist of someone else who does not know, but who is willing to try to find out.

The nearer the critic approximates in intelligence and in sensibility to the artist the more use he will be to him. In intelligence and in sensibility, yes; but not in experience. If the artist and the critic share a great deal of their past histories it may well happen that something may be intelligible or significant to the critic in virtue of mere personal accidents. If they have much in common in their lives, it may be specially difficult for the critic to distinguish between genuine art and mere aesthetic reproduction; and a great many more so-called works of art are, as a matter of fact, aesthetic reproductions than is often realized. They aim at producing in the spectator, not an artistic reaction to a work of art, but an echo of an aesthetic experience. They take him back to a familiar haven. Mr Hilaire Belloc has composed a little poem to serve as an illustration of this point, putting in 'Tumty' so as not to offend any school or river; fill it in as you will.

> Ah, years ago, but I once was there
> And I wish I was there again,
> By Tumty River, and Tumty Weir,
> Along with the Tumty men.

The point may perhaps be made clearer in this way. Consider the following detached phrases and give yourselves up to the imagery they suggest.

> Sun-warmed strawberries, sheltered by dewy leaves.
> The burr of a cider-press in a Brittany farm-yard.
> The rhythmic creak of rooks' wings above a ploughed field at dusk.
> Buds on a lime tree seen against sooty roofs in early May.
> The thin thread of a new-born lamb's complaint.
> The swish of milk in the pail and the smell of a well-kept byre.

Now, there is no *art* in these phrases. They stir you, if they stir you at all, to the renewal of past aesthetic activity, or perhaps to an emotional activity that is not aesthetic. But it is pitifully easy to string such phrases together in a well-marked rhythm ("the kind of lilt which you get from an engine wheel with a flat in it bumping down an incline"—to quote Mr Belloc again) and to delude yourself and others into thinking that you are an artist.

Near Folly Farm in April,
Catkins, red-tipped are found.
And while the thrushes gay trill,
Pale cowslips star the ground;
And the wild strawberry bud,
(And God's own mud)
On God's own earth abound.

Near Folly Farm the Cotswold Hills
Climb up to greet God's heaven.
Ah, their clay it is streaked with a hundred rills,
And the stars on their summit are seven!
And the ruts in the lane
Lie deep-scarred as with pain
By the carts that the farm-lads have driven.

In Folly Farm we Georgians stay,
Beneath the wind-swept trees.
A score of us have found the way
To fill our rhymes with breeze;
For God's thinnest stuff,
If it's fresh-air enough,
Gets the public we're out to please.

To have stayed at Folly Farm together is not the best bond for artist and critic. Indeed, the most trustworthy critic will probably be a stranger. He must be a kinsman, if you will, but a kinsman from a far country, who sees our familiar streets with new eyes, and listens to the familiar rumble of our traffic with new and wondering ears. And yet a kinsman—who finds himself at home in a fair city. It is a case of Plato's philosopher, perhaps; of the philosopher, or the just man, who recognizes goodness at sight, and feels at home with it. The good critic recognizes good art, knows it, feels at home with it. The city of art is a free city needing no shibboleths to gain admittance. He that hath feet to bear him thither, let him come. And the rare and perfect critic speeds straight to the innermost temple. And there the artist waits for him in silence, knowing that if his kinsman finds his way without a stumble, or a detour, the city is well founded and well planned. His coming brings to both a Felicity comparable only to that of those who dwell in, or find their way to, that city of which Plato spoke, whose pattern is laid up in heaven for him who wishes to behold it.

> It is a good thing to be happy alone; it is better to be happy in company.... Nothing can be our happiness but that alone which we rightly apprehend.... We are more happy in communication than in enjoyment, but only that communication is enjoyment: as indeed what we give we best receive. For the joy of communicating and the joy of receiving maketh perfect happiness.

So says Traherne, and with his words we may conclude, for what is the exercise of the critic but to receive and to communicate that which the artist has himself communicated and received?

(*Manuscript received* 15 *March*, 1928.)

# PUBLICATIONS RECENTLY RECEIVED.

*Brain Mechanisms and Intelligence: A Quantitative Study of Injuries to the Brain.* By K. S. Lashley. Foreword by H. M. Adler. 1929. Pp. xiv + 186. 11 Plates. University of Chicago Press. $ 3.00.

This book is a summary of Prof. Lashley's recent work on the effects of injuries to the brain in the rat, and a discussion of the bearing of the results on theories of cerebral function, intelligence, and learning. The main experimental results described in the book are as follows:

Destruction of any part of the cerebrum leads to a reduction of the rate at which a rat learns a given maze, and the amount of the reduction is roughly proportional to the amount of tissue destroyed and independent of the locus of the injury. The performance of maze habits formed before the lesion is produced, and the retention of maze habits formed afterwards, are affected in a similar way. An injury of a given extent produces a relatively larger effect on complicated maze habits than on simple ones. There is no detectable qualitative difference in the maze behaviour of rats with injuries to different parts of the cortex; but in all cases the operated animals show a "general inadequacy in adaptive behaviour" comparable with general dementia in man.

The results obtained from experiments on brightness discrimination and on the solution of a 'double-platform' problem box were somewhat different. In these cases it is found that the removal of any quantity of the cerebrum up to about 50 per cent. of the whole has little or no effect on the ability to acquire the habit. But if the habit is formed in an intact animal, removal of the occipital lobes destroys the brightness discrimination habit, and removal of the frontal lobes destroys the problem box habit. In either case the habit can then be re-learnt at a rate about equal to that of the original learning, and it does not then become localized in any particular cortical area. Lashley considers that the difference between these results and those for maze learning is probably due to the greater complexity of the maze habits.

In other experiments it was found that cutting various association tracts in the cortex had little or no observable effect on behaviour.

The difficulty of reconciling these results with current theories of cerebral localization and habit formation is obvious. Lashley argues that his results are quite inconsistent with any of the theories which attempt to explain learning in terms of alterations in synaptic resistance. His own tentative theory of cortical functioning is in terms of physiological gradients rather than nerve impulses. There is a good deal of evidence that in higher animals the localization of function in the cortex is much better defined than in the rat; but, in a critical review of some of this evidence, Lashley argues that even in the highest animals localization is not nearly so strict as is usually supposed.

Lashley's results seem likely to be of revolutionary importance, and all those interested in cerebral physiology will welcome this concise account of a large section of his work.

*Biological Principles.* By J. H. Woodger. London: Kegan Paul (International Library of Psychology, Philosophy & Scientific Method). 1929. Pp. xii + 498. 21*s.* net.

This is a deeply interesting book though it is somewhat unnecessarily long. After a general introduction which deals with the historical growth of biological thought and methods, the author discusses at length the data of natural science and the principles of systematization of scientific knowledge. Naturally this section is largely

philosophical in bent. Phenomenalism and its alternatives are considered in the light of recent controversies, the categories of substance and causation are passed in critical review, and an attempt is made to say what are the postulates and demands of natural science and what is contributed by subjective factors. Problems of biological knowledge are next considered. Here the relation of vitalism and mechanism plays a large part, and other antitheses dealt with are those of structure and function, organism and environment, preformation and epigenesis in individual and racial development, teleology and causation, and mind and body. On the whole the main conclusion is that the sound development of biological science depends less upon getting biologists who can apply exact experimental method than upon producing biologists who can *think*.

*Physiology of the Central Nervous System and Special Senses.* By N. J. Vazifdar. Calcutta: Thacker, Spink and Co. 1929. Fifth edition. Pp. vii + 301.

This book is a carefully arranged epitome of the main facts of the physiology of the central nervous system and special senses. The fact that it has reached its fifth edition shows that it serves a useful purpose in giving so much information in such a small compass. There are thirty diagrams, but this is hardly an adequate number, and some of them are not very clear. There are a large number of misprints; and, especially in the chapters on the special senses, a number of inaccurate and obscure statements. For instance, it is stated on p. 294 that "the velocity being proportional to the intensity of the sound, loud sounds travel more rapidly than feeble ones"; and on p. 276 the Purkinje phenomenon is described as follows, "The effects of change in intensity of light on the colours is rendered obvious by the fact that when the intensity of light is diminished, the brightness of the spectrum is shifted from the yellow to the green, and a coloured object appears colourless," no further explanation being given. Faults such as these could easily be corrected if the text were revised in a new edition; and if this were done, and more diagrams were added, the usefulness of the book would be very much increased.

*An Introduction to the Study of the Nervous System.* By E. E. Hewer and G. M. Sandes. Foreword by Prof. Winifred Cullis. London: William Heinemann. 1929. Pp. xi + 104. 21*s*. net.

The title of this book is misleading, since the authors have not attempted to write a general account of the anatomy or physiology of the nervous system. As is explained in the Preface, "The book...is intended to stress particularly the points which we, as teachers, find are not sufficiently clearly expressed elsewhere." "Minute structure and function have been considered together, but no description of gross structure has been given, as this can be found so admirably expressed in many modern text-books."

The tracts in the spinal cord, the deep connections of the cranial nerves, and the connections of the lower brain centres are described at some length, and illustrated by very clear diagrams. Nerve cells and fibres, the cerebro-spinal fluid, the autonomic nervous system, reflex action, co-ordination of movement, and localization of function in the cerebral cortex are treated more briefly; but in all cases a great deal of information is given in a condensed form, and the diagrams are always admirable.

Psychologists interested in the localization of function in the nervous system, as well as medical students, will find this a very valuable supplement to other works of reference.

*Contributions to Psychiatry, Neurology and Sociology.* Dedicated to the late Sir FREDERICK MOTT. Edited by J. R. LORD. London: H. K. Lewis. 1929. Pp. xii + 401. 21*s.* net.

It is impossible, within the limits of book notices published in this *Journal*, to do more than call the attention of all of its readers who are interested in psychiatrical problems to the publication of this magnificent work. The book, which is the work of colleagues of the late Sir Frederick Mott, is published by the Mott Memorial Committee for whom Dr Lord has acted as a most efficient editor. It is right and natural that most of the studies should be concerned with neurological and anatomical questions. But all of these are in first-rate hands, and there are in addition many striking theoretical and clinical studies of functional, mental and nervous disorders. The book deserves and will no doubt receive a ready welcome from a varied scientific public. No better memorial of a life devoted to scientific investigation in psychopathology and allied fields could have been devised.

*An Historical Introduction to Modern Psychology.* By GARDNER MURPHY, Ph.D. London: Kegan Paul. 1929. Pp. xvii + 470. 21*s.* net.

A more satisfactory addition to the International Library of Psychology, Philosophy and Scientific Method than a history of the development of experimental psychology could not be imagined. There will be many students of psychology who will be extremely glad to have this volume at their disposal. Dr Murphy has successfully selected most of the contributions which ought to figure in his study, though possibly he has laid more stress than is justified on the works of American authors such as Woodworth, to the neglect of Englishmen like Rivers and Myers. Apart from this there are few serious criticisms: Dr Murphy writes a more pleasant style, and is clear and intelligible under the worst of circumstances.

The book falls naturally into three parts; the history of the pre-experimental period, that of the early stages of laboratory psychology, and the final account of modern views and problems. In the first part attention is drawn to the separation by the growth of learning, of those aspects of mental science which are susceptible of experimental treatment, away from original philosophical and logical fields. This development was mainly due to the English critical philosophy and its subsequent culmination in the associationism of the nineteenth century. When this associationism waned after reaching its climax new impulses were received from the work of biologists, and from physiologists in particular. Modern psychology is almost founded upon a combination of the analytic and introspective method, with the objective attitude of the experimentalists. McDougall has emphasized this fact already. Perhaps in this volume Dr Murphy is inclined to over-emphasize the importance of the external or physiological approach. As he is writing a history of the development of experimental psychology this is necessary; but as he calls it a history of modern psychology, his attitude is misleading. Behaviourism has shown by its failure that psychology will not advance by the rigid exclusion of the one aspect; both are necessary and essentially complementary to each other. Dr Murphy hardly seems fully to understand the point of view of observational psychology that this is not the same as, or even an unscientific version of, the truly experimental method: there are some aspects of the subject that will never lend themselves to laboratory treatment. The all round psychologist will always have to be a field naturalist in some degree. The psychological field naturalist ought to be able to claim a distinguished place among scientific workers. Dr Kluger's account of the modern movements in Germany, an appendix to the history, is disappointing.

*The Psychological Register.* Edited by Carl Murchison and others. Worcester, Massachusetts: Clark University Press (London: Humphrey Milford). 1929. Pp. ix + 580. 27*s.* net.

This is a *Who's Who* for psychologists of the world. It gives date of birth, past and present official position, honours and publications of accredited psychological experts. All things considered it is a remarkably complete work of reference. Many of the lists of publications are nothing short of stupendous. There are doubtless some omissions and some inaccuracies, but Prof. Murchison has carried out a most difficult task with conspicuous success, and has produced a work that every psychological department must regard as a necessary possession. The editor promises to keep the work up to date by frequent revision.

*Manual of Psychology.* By G. F. Stout, M.A., LL.D. Univ. Tutorial Press. Fourth edition. Pp. xix + 680. 12*s.* 6*d.* net.

This fourth edition of Prof. Stout's *Manual*, revised by the author and Mr C. A. Mace, contains some changes. The most important of these are the following: (1) Prof. Stout's general theory of the relation of thought to sentience with its application to the problems of perception has been re-stated and amplified. This has involved a radical revision of parts of Book III, Part II, dealing with the growth of our perception of the external world. (2) A number of amplifications have been introduced into those parts of Prof. Stout's psychological theory which have a bearing on the problems of the 'Philosophy of Mind.' These put more clearly his rejection of the views of the Atomistic school, and demonstrate certain similarities which exist between his views and those of the Gestalt school.

*The Mechanics of the Life of the Soul. An Introduction to Physiological Psychology.* By N. A. Kabanow. Moscow. 1928. Pp. 1–160.

This is a popular book, treating psychology mainly from the point of view of conditioned reflexes. In the first part of the book the connection between nerve physiology, brain anatomy and the simpler mental activities is quite ably worked out, but the greater part of the book is taken up by a discussion of emotions, thought, will, etc., purely from the point of view of conditioned reflexes and association and in a completely dogmatic manner. The impression of dogmatism is increased by the fact that throughout the book not a single author is named, nor is any evidence given. The problem of consciousness is not touched.

*Creative Imagination.* By June E. Downey. London: Kegan Paul. 1929. Pp. viii + 228. 10*s.* 6*d.*

Prof. Downey takes the problems of the descriptive psychology of literature, and treats them in a popular manner which is easily understandable. It is a disappointment only to the psychologist that she does not attempt a theoretical account of the part played by the functions that she analyses. She does this neither for the creative nor for the interpretative imagination, with which she is more concerned in fact. The book is merely an account of the general character of the interpretative imagination as it appears among differently constituted persons in the sphere of literary appreciation; it is not really a discussion of the creative aspects of imagination at all. It is a good account for the general reader, but much less so the specialist.

*The Future of an Illusion.* By SIGMUND FREUD. The Hogarth Press. 1928. Pp. 98. 6*s*.

This is the fifteenth volume of the International Psycho-Analytic Library, edited by Ernest Jones. In it Freud reviews briefly what he considers to be the psychology of the evolution of religion, taking each aspect in a fresh chapter. It is his opinion that religion, which is an illusion, can never provide the satisfactions fully that are expected of it; and that, science is bound to be substituted for it in the future, if reason is to gain control over instinct. Culture and civilization place inhibitions of a serious character on the natural tendencies of man (towards incest and to murder), and so bring deprivation to their members. Religion excuses culture for its misdeeds in these directions, provides a series of substitute satisfactions, and gives such a soothing account of the meaning of existence that all our natural terrors are relieved and our problems solved for us. We believe religious doctrines simply because our forefathers have done so for centuries, and because of our tendency to adhere to that which is pleasing and to behave 'as if' it is true. But the development of mankind is such that reason inevitably will come to govern instinct: that it does not fully now does not show that it will not some day: and then the illusion of religion will be superseded by a science which is true. Freud is the bringer of this new light, and Psycho-Analysis his torch.

*Die Methoden der pädagogischen Psychologie.* Von W. J. RUTTMANN. Halle a. S.: Carl Marhold. 1930. xii + 487 S. 19.75 R.M.

This first volume of a new general treatise on educational psychology covers a vast field with very considerable success. Prof. Ruttmann has really attempted a comprehensive survey of the whole of psychology, so far as current methods are concerned, in its bearing upon educational problems. The material is very clearly arranged. A brief introduction leads to a statement, with some illustration of discussion of the main division of psychology: (*a*) general, (*b*) individual, (*c*) genetic, (*d*) social, and (*e*) border line topics, with particular reference to the physiology of the central nervous system. Next, different methods are discussed and illustrated in some detail. Special sections deal with: experimental method; the method of 'exploration,' which includes the application of tests; the introspective method, the statistical method, and the 'personal' method, which includes a study of character, temperament, 'types' and the like. It is inevitable that a number of the topics considered should be dealt with in a somewhat scrappy manner, but a prodigious amount of information is imparted. The book is enlivened by 158 illustrations, and every section has a long list of references, some of which are annotated.

*Research in the Social Sciences.* Edited by WILSON GEE. New York: The MacMillan Company. 1929. Pp. x + 305. 8*s*. 6*d*. net.

This book contains nine lectures developed before the Institute for Research in the Social Sciences at the University of Virginia, early in the autumn of 1926. The subjects and lecturers were as follows: Sociology, by R. E. Park; Economics, by A. A. Young; Anthropology, by Clark Wissler; Statistics, by R. E. Chaddock; Psychology, by R. S. Woodworth; Jurisprudence, by Roscoe Pound; History, by A. M. Schlesinger; Philosophy, by John Dewey; Political Science, by C. A. Beard. As a contribution to a definition of varying objectives and methods the volume is one of considerable interest.

*The Growth of Reason: A Study of the Rôle of Verbal Activity in the Growth of the Structure of the Human Mind.* By Frank Lorimer. London: Kegan Paul (International Library of Psychology, Philosophy and Scientific Method). 1929. Pp. xii + 230. 18*s.* 6*d.* net.

The main value and interest of this book are that scattered throughout its pages are numerous first-hand records of the verbal activity of young children. The author tries to show how verbal activities are conditioned, developed and combined in the growth of reasoning. The last two chapters, dealing with Philosophical Theory and Logic in Society, are too general and vague to be of very much service. There is a good selected bibliography. The author has much to say also about symbols and their social significance, but here his work suffers from a lack of concrete illustration, and a great opportunity has, from a psychological point of view, been lost.

*The Measurement of Nervous Habits in Normal Children.* By Willard C. Olson. University of Minnesota Press: Institute of Child Welfare, Monograph Series, No. III. 1929. Pp. xii + 97. $ 2.00.

The author discusses the criteria of nervous habits in children: presents a classification of such habits; develops observational methods for their study; considers the expression of the measurements so obtained and their reliability; investigates the incidence and distribution of the habits and their genesis; and finally, critically studies various test techniques that have been used for their discovery. The whole monograph is clear and definite, and is a most interesting piece of pioneer work.

*Personality Adjustments of School Children.* By Caroline B. Zachry. London: Charles Scribner's Sons. 1929. Pp. xiii + 306. 7*s.* 6*d.* net.

The book is intended to be definitely practical. Its method is to take a particular type of difficult child, give a detailed case history and discuss the special difficulties involved in the light of the special case chosen. In this way the following types are dealt with: the troublesome child; the over-conscientious child; the "child with a polyglandular difficulty"; the over-dependent child; and the over-anxious child. Two additional chapters deal with elements of personality and their development, and personal adjustment and the school. The large number of modern books of this general type will perhaps soon present somebody with an excellent opportunity of studying the over-anxious teacher.

*General Psychology for College Students.* By C. N. Rexroad. New York: The MacMillan Company. 1929. Pp. xv + 392. 8*s.* 6*d.* net.

Here is a general text-book which is about as consistently behaviouristic as it is possible to be. The position is clear enough. Whether people are conscious or not makes no difference to anything. Consequently speaking in terms of mental processes has no place in an explanatory science. Naturally enough the author cannot escape the use of terms like 'attention,' 'choice,' 'bragging,' 'worry,' and so on, which usually are held to indicate either conscious factors or processes of consciousness. But he is ready to try to define them all in terms of obvious bodily reaction. He gives a tremendous lot of questions and exercises at the end of the different chapters, and many of these are well selected.

The book is well and clearly written. That to anybody who does not adopt its point of view the terminology should seem often exceedingly roundabout and cumbrous cannot be avoided. It is as good an attempt as has yet been made to cover nearly the whole field of normal psychology by an exclusively behaviouristic method.

*An Approach to Composition through Psychology.* By PHYLLIS ROBBINS. Harvard Studies in Education, vol. 12. Harvard University Press (London: Humphrey Milford). 1929. Pp. xvii + 271. 13*s.* 6*d.* net.

An introduction divides "the workings of the human mind" into observation, imagination, reflection. Schemes indicating what each of these can do are given. A writer, it is held, must know not only how his own mind works, but how the minds of other people work also. So information is given as to how we become acquainted with other people. The predominant part played by words in all this is described. Then the volume gives masses of passages of prose and poetry, and a lot of pictures trying to urge the student to put into psychological terms how they impress and what they repress. Whether anybody will learn to write better for the study of this book it is hard to say. The treatment is rather jerky and disconnected. But the author has lots of ideas, any amount of enthusiasm, and is very much alive. There is a formidable bibliography.

*The Psychology of the Infant.* By SIEGFRIED BERNFELD. London: Kegan Paul (Library of Educational Psychology). 1929. Pp. xi + 309. 15*s.* net.

This volume, well translated by Rosetta Hurwitz, is a comprehensive monograph upon the development of the life of the infant and the very young child. Dr Bernfeld shows a wide and accurate knowledge of previous studies, many of which are difficult to obtain, and has also many first-hand observations to record. The result is a book of great value, which should take a good place amongst the best studies of infancy. There is unfortunately one drawback, and that is a tendency to mix speculation with evidence. Read with care the work will add to knowledge and stimulate thought.

*Havelock Ellis: Philosopher of Love.* By HOUSTON PETERSON. Illustrated. George Allen and Unwin, Ltd. 1929. Pp. ix + 432. 18*s.* net.

This is a fascinating book: Havelock Ellis is a great subject, and Houston Peterson a talented biographer; like a good accompanist, he knows how to efface himself. It has few faults, and these are slight. If it is a little sentimental, that does not matter seriously; and if the list of Ellis' published works is somewhat in bad taste, yet it might be extremely useful, and certainly is most interesting and calls up immediate admiration.

It would be hard to find a man among scientific workers of whom a biography could be made more interesting. Ellis is a philosopher without being a controversialist; a psychologist who never puts forward hollow speculations or wastes energy in idle polemics against opponents. He is a great scientist, an ideal to all followers of that calling, for he has spent his life in a patient and painstaking investigation of his problems, leaving no stone unturned that might lead to the discovery of a new aspect.

In belonging to the philosophers who are silent rather than with those who talk he is unusual, even for a scientist; his philosophy is an amazing achievement in the very best tradition of humanism—there is little talk and much fulfilment. Having eliminated antiquated moral notions from his outlook in early years he has systematically worked for the substitution of these in others by insight and enlightenment: thus his conception of the scientific outlook is not one of narrow prejudice, nor of mere rebellion from traditional forces, but of the widest sympathy with human beings and the deepest understanding.

Thus it does not seem surprising to find in Ellis a large expression of the artist, though few people know both sides of his activity. He was responsible for the origin of the Mermaid Series, and thus helped to make Elizabethan literature accessible to the general public. He also edited the Contemporary Science Series, and contributed a volume. But he is a poet, and there are many literary works and critical essays

from his pen, especially concerning the Spanish painters and dances, and the novelists of France.

Ellis dedicated his life to the study of the problems of sex-psychology: in his attitude to these he contrasts strongly with his contemporary, Freud. He is no iconoclast who sees in human traditions nothing but offences against nature, and wishes to dissolve all culture upon which man has set value if he can so much as say it is illusion. Freud is unhappy unless he is engaged in intellectual warfare: Ellis refuses controversy. Unlike Freud, he wishes patiently to search out all that is bad—that is inhuman and anti-human—in civilized life, and to replace it by what is better. Never will his method degenerate into attack and destruction; it will always be a sympathetic synthesis.

It is in the biography the man that interests: what he valued and set his heart on; his outlook, balance and fitness of judgment; his simplicity and power of undeviating persistence.

*Madness in Shakespearian Tragedy.* By H. Somerville. London: The Richards Press, Ltd. 1929. Pp. 207. 6*s.* net.

It is difficult to place this volume correctly. If it is to be regarded as an illustrative study of the characteristics of certain mental abnormalities it is interesting and accurate work. If it is to be supposed that the study throws very much light upon the tragedies dealt with there is room for plenty of doubt. Is Shakespeare supposed to be making a definite contribution to the investigation of several types of insanity which have since his time received classificatory names? That seems unlikely. Still, Mr Somerville's book is undoubtedly very well written, and it may help the student to remember more accurately the features of the forms of insanity dealt with. Mr Wyndham Lewis provides an acute and amusing preface.

*Human Nature and its Remaking.* By W. E. Hocking. New Haven: Yale University Press (London: Humphrey Milford). 1929. Pp. xxvi + 496. $ 4.00.

The second edition of this book gives Prof. Hocking an opportunity "to take account of recent discussions of the place of instinct in human nature, of certain theoretical aspects of the Freudian views, and of Prof. Dewey's notable book on *Human Nature and Conduct*." As is well known, the work is a serious and important study of human equipment and what can be made of it.

*Some Prominent Characteristics of Human Nature and a New Conception of God.* By Adane. London: C. W. Daniel Co. 1929. Pp. 166. 6*s.* net.

The prominent characteristics in question consist of numerous emotions, emotional tendencies and sentiments, of which the author constructs a very long list. These are descriptively studied throughout more than two-thirds of the book, with a very long disquisition indeed on Love. The description is not very original and not in the least exciting, but no doubt much of it is true. The part which presents "a new conception of God" seems rather muddled, and in every case it has little to do with psychology.

*A Preface to Morals.* By WALTER LIPPMANN. London: George Allen and Unwin, Ltd. 1929. Pp. viii + 348.

This popularly written book appears to have had a great success in America. In the first 139 pages Mr Lippmann argues that belief in a supernatural order behind the world has been undermined. In the next 67 pages he appears to argue that the "humanistic view," which "rests on human psychology," is destined to restore all the value of what has been lost, by establishing "high religion." It is not altogether clear what "high religion" is, but at least it is an aristocracy of spirit and "the art and theory of the internal life of man." In the last 117 pages Mr Lippmann works out the meaning of "high religion" in regard to a number of current social problems. Here he says a lot of sensible things, both clearly and well.

*The Idea of Value.* By JOHN LAIRD. Cambridge University Press. 1929. Pp. xx + 384. 18*s.* net.

Although this book is primarily philosophic in its mode of approach and presentation, it contains a great deal to interest the theoretically minded psychologist. Prof. Laird is sympathetic to some of the contemporary movements in psychology, and particularly to those which make use of the notions of pattern, *gestalt*, or schemata. He clearly points out the limitations of these notions, however, in regard to his main problems. The writing is uniformly interesting and lively.

*A Manual of Ethics.* By J. S. MACKENZIE. London: W. B. CLIVE: University Tutorial Press, Ltd. 1929. Sixth edition. Pp. xii + 426. 9*s.* 6*d.*

The sixth edition of this well-known text-book has been fully revised. In particular Dr Mackenzie deals with criticisms by Dr G. E. Moore and the late Dean Rashdall of his earlier editions. Thus fortified, the book will no doubt continue for a long time to run its well-deserved successful cruise.

*The Life and Work of Mrs Piper.* By ALTA L. PIPER. London: Kegan Paul. 1929. Pp. xi + 204. 7*s.* 6*d.* net.

This book gives an attractive and straightforward account of the life and achievements of Mrs Piper. It is not critical, but it will be welcomed as an authoritative record. The volume contains several pleasant anecdotes of the various illustrious persons with whom Mrs Piper has at various times been connected.

# PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY

## GENERAL MEETINGS.

November 16, 1929. "Mental Energy." By Mr C. Fox, M.A.

December 14, 1929. "The Trust of Love." By Dr A. F. Shand.
"Phenomenal Thing-Moments." By Dr Kardos.

## SECTIONAL MEETINGS.

### Medical.

October 23, 1929. "Psychology and Medicine." By Dr Henry Yellowlees.

November 27, 1929. "Life and Death Instincts." By Dr C. P. Blacker.

December 18, 1929. "Some Difficulties in Analytical Theory and Practice." By Dr T. A. Ross.

### Education.

October 7, 1929. "The Incidence of Mental Defect in the School Population." By E. O. Lewis, Esq., D.Sc., L.R.C.P.

November 4, 1929. "Creative Work and its Effect on Appreciation." An Experiment in the Teaching of Poetry. By Miss K. Leopold, B.A.

December 2, 1929. "Child Guidance Clinics." By Miss Lucy Fildes, Ph.D.

### Industrial.

October 12, 1929. "Some Inner Conflicts in Industry." By Mary Barnett Gilson, M.A.

November 15, 1929. "Some effects of Low-Frequency Vibration." By Mr R. J. Bartlett, M.Sc.
"Factors in Unproductive Time." By Mr S. Wyatt, M.Sc.
"Some Problems of Vocational Guidance." By Mr Angus Macrae, M.B.

December 17, 1929. A Symposium on "Noise" was arranged, to which the following individuals contributed: Miss I. Lorrain-Smith, M.A.; Miss Katherine Pollock, B.A.; Dr H. M. Vernon, M.D.; Major W. S. Tucker, O.B.E., D.Sc.; Dr Millais Culpin, M.D., F.R.C.S.

### Aesthetics.

November 22, 1929. "Spengler's Aesthetic." By Mr J. M. Thorburn, M.A.

BRITISH PSYCHOLOGICAL SOCIETY. (SCOTTISH BRANCH.)

SESSION 1929–1930.

November 2, 1929. "What is the Super-ego?" By Dr W. R. D. FAIRBAIRN.
"Experimental Aesthetics in relation to the Pictorial Artist." By Mr T. ELDER DICKSON.

December 7, 1929. "Totem and Tabu in Scottish Superstitions." By Dr E. H. CONNELL.
"Some Experiments on Perception." By Dr R. H. THOULESS.
"The British Association in South Africa." By Dr S. DAWSON.
"Impressions of American Psychology 1929." By Dr J. DREVER.
"Impressions of the Psycho-Analytic Congress at Oxford." By Dr W. R. D. FAIRBAIRN.

January 25, 1930. "What are the Primary Colours?" By Dr J. DREVER.
"The Employment of Motion-Study in an Investigation of Skill." By Miss A. SHAW.

March 1, 1930. "The Measurement of Social Adaptability." By Mr D. KENNEDY-FRASER.
"The Present Position of Psychotherapy." By Dr H. YELLOWLEES.

# COMMITTEE FOR RESEARCH IN EDUCATION.

With a view to facilitating the general co-ordination of research in education, the Committee has made its annual enquiry as to researches in progress, or planned, at the various Psychological Laboratories and Training Departments.

The following is a summary of the replies received:

## RESEARCHES IN PROGRESS OR PLANNED, DECEMBER 1929.

### ABERYSTWYTH. University College (Dept. of Education).

Enquiries into conditions of Specialization in Teaching and Subject Requirements in Secondary and Central Schools.

Investigations into effect of previous Teaching Experience, Intelligence, Academic Capacity and other Factors on success in Teaching.

Enquiries into the Psychological and Pedagogical Aspects of the Problem of Bi lingualism in Wales and other Bilingual Countries.

### BANGOR. University College. N. Wales.

"Intelligence" and Ability in Teaching.

### CARDIFF. University College. Wales and Monmouthshire.

An Investigation of the Characteristics of Adolescence.

The Use of Non-Linguistic Intelligence Tests.

Experiments on Reading in relation to Literary Appreciation.

### BIRMINGHAM UNIVERSITY (Education Dept.).

A Statistical Enquiry as to the Correlation of School and University Successes.

Tests of Higher Mental Processes.

Studies in the Psychology of Early Childhood.

Reasons for the Choice of Occupations among Secondary School Pupils.

An Experimental Enquiry into the suitability of the Otis group tests of Intelligence for use with Matriculated Students.

A Study of various Members of the Group of Algae known as Heterokontae.

The Development of Humour in Children.

Preferences of Children and Adults in Poetry and Prose Literature.

Errors in English Composition as revealed by Essays written by Higher School Certificate Candidates and University post-graduate Students.

Formal Training: an historical and critical review with a re-examination in the light of Modern Psychology.

The Motives of English State Education considered in relation to the problem of Educational Equality.

UNIVERSITY OF DURHAM. Armstrong College. Newcastle-upon-Tyne.

Investigation into the Merits of Different Systems of Examinations.
Investigation into the Value of School Record Cards.

UNIVERSITY OF EDINBURGH. George Combe Psychological Laboratory.

The Motivation and Accessory Concomitants of Delinquency, with especial regard to Adolescents.
The Speed Factor in Intelligence.
Variations in Suggestibility with Age, Sex, and Grades of Intelligence.
A Psychological Study of Art, with special reference to Pictorial Art.
A Psychological Investigation of the Perception and Memory of Time Relations and the Development of Time Concepts in High School pupils.

UNIVERSITY OF EDINBURGH (Dept. of Education).

The Improvement of Methods and Materials in Native African Schools.
The Relation between Intelligence and the Differential Birth Rate.
Mathematical Enquiries into Problems of Sampling.
The use of "Path Coefficients" in investigating the Factors of (*a*) Ability in Algebra and (*b*) Ability in English.
Factors in Teaching, Learning and Testing Spelling.
The Sense of Causal Relationship in History.
Proper Weighting of Marks in College Examinations.
Follow up Enquiries of Children of Known Intelligence.
A Silent Reading Test.

UNIVERSITY COLLEGE OF THE SOUTH WEST. Exeter.

An Evaluation of the Qualities of a Good Teacher by a Survey of Professional Opinions.
An Attempt to Measure Children's Interests.

UNIVERSITY OF LEEDS (Dept. of Education).

An Enquiry into the value of Films in the Teaching of History.
A Psychological Study of Physically Defective Children.
A Study of Mental Inertia.
An Experimental Study of School Children with regard to some Racial Mental Differences.
An Experimental Enquiry into the Value of Silent Reading in Modern Language Teaching in Schools.
The Vocational Guidance of Boys in a Central School.

LONDON DAY TRAINING COLLEGE.

Vocational Guidance in Secondary Schools.
Methods of Teaching Dull and Backward Children.
The Psychological Basis of Intonation.
The Transfer of Training in Arithmetical Reasoning.
Psychological Problems in the Teaching of Geography.
The Psychological Approach to Religious Education.
The Value of Broadcasting in Schools.
The Psychology of Learning French.
Education in Ceylon.

UNIVERSITY OF LONDON. BEDFORD COLLEGE.

Children's Thinking.
Study of the part played by Imagery in Learning (continuation).

UNIVERSITY OF LONDON. KING'S COLLEGE.

The Psychology of Evidence (among young delinquents).
The Influence of Conative Control (continuation).
Memory (Retentivity) (continuation).
Investigation into the Possibility of Tests of Culture.
The Nature of Errors in Tachistoscopic Perception. (Reading.)
Character, Will and Emotional Characters in Children.
On Conation and Volition.
Aesthetic Appreciation of Literature.
Influence of Conation on Mental and Manual Performances.

UNIVERSITY OF LONDON. UNIVERSITY COLLEGE.

Factors and Tests of Mathematical Ability.
Effects of Bi-lingual Education on Mental Development.
Perceptual Tests of "g."
Verbal and Perceptual Tests of Ability.
The Relation of Ability and Perseveration to Speed.
Development of Concepts in Young Children.
Speed for various kinds of Reaction.
Cyclothymic and Pyknic Types.
Powers of Memory and their Inter-relations.
The Perceptions of the Blind.
Formal Training as instanced in Mathematics.
Experiments on Different Ways of learning German.
Factors and Tests of Musical Ability.
The Nature of "Confusion."
Individual Differences and Tests of Memory.
The Imagination of Young Children.
Ability to estimate Character.
The Mind of the 3-year old.
Oscillation of Mental Output.
Mathematical Basis of the Theory of "Two Factors."
The Functions of Images.
Research into Sources of Imagination.
Survey of the Chief Features in Ability and Character, Normal and Abnormal.

UNIVERSITY OF MANCHESTER.

The Influence of the Distribution and the Length of Rest Periods upon Efficiency in Learning.
Auditory Imagery, with special reference to the possibility of investigating Individual Differences in Auditory Eidetic Imagery.

NATIONAL INSTITUTE OF INDUSTRIAL PSYCHOLOGY.

Study of the Problems of the Vocational Guidance of Elementary School Children.

The follow-up work in connection with the Institute's London experiment has been completed and the results are now being analysed. A report covering the whole enquiry is being prepared.

Study of Vocational Guidance Problems. The use of Vocational Tests at different ages and in different environments.

The Institute's Fife experiment is being continued. It provides for a comparison between the vocational problems of rural and urban children and for a systematic study of the child from the vocational point of view from the age of eleven. A proportion of the children studied will eventually enter employment after several years of post-primary education at secondary and other schools.

Study of Assembling Tests of Mechanical Ability.

A report on this subject will be published shortly.

Study of Tests of Manual Dexterity.

A report on this subject will be published shortly.

Study of Objective Tests of Temperament.

Study of Factors underlying Assembling Work.

Study of the Causes of Intermittent Employment among Boys.

The Physiology and Psychology of Colour Matching.

Study of the Problems of Blind Workers.

An investigation for the National Institute for the Blind.

Investigation of Fluctuations in the Efficiency of Workers and their Determining Conditions.

Study of Perseveration as a probable Factor determining Temperament, affecting Output, Fatigue, etc., and causing Mental Strain in persons engaged in unsuitable vocations.

READING UNIVERSITY.

Children's Judgments of Space Relations as a Factor in the Teaching of Geography.

SHEFFIELD UNIVERSITY.

The Study of Children's Vocabularies.

Boundary Conditions for Correlation Coefficients.

An Investigation into the Relative Value of Methods of Teaching Science in Selected Central Schools.

A Study of Mathematical Reasoning among Senior School pupils.

UNIVERSITY OF ST ANDREWS (EDUCATION DEPT.).

The Development and Cultivation of Literary Appreciation.

Studies of Child Development.

PSYCHOLOGICAL LABORATORY.

An Investigation into "Factors of Control" in Controlled Association.

Investigations Relating to the Influence of Noise upon Mental and Physical Work.

Continuation of Previous Investigation into the Conditions of "Natural" Rates of Work.

The Influence of Special Incentives upon Curves of Learning.

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3. The Society shall be responsible for the publication of *The British Journal of Psychology*.

### MEMBERS AND SECTIONS.

4. The Society shall consist of Honorary Members, Ordinary Members and Sectional Associates.

5. Persons of scientific distinction who have contributed to the advancement of Psychology shall be eligible for election as Honorary Members.

6. Honorary Members shall be entitled to all rights and privileges of membership.

7. Honorary Members may be elected from time to time on the nomination of the Council at any General Meeting of the Society.

8. The Council shall be empowered to institute Sections of the Society, each Section being concerned with a special branch or aspect of Psychology.

9. Election to a Section or Sections shall *ipso facto* constitute election to the Society as a whole and entitle those so elected to receive notice of and to take part in all General Meetings of the Society in addition to the meetings of the Section or Sections to which they belong.

10. Sections shall have the right of electing Sectional Associates who shall receive notice of and shall be permitted to attend all meetings of the Section or Sections to which they belong. Such Sectional Associates shall however take no part in the Government of the Society or the Sections, and they shall not receive *The British Journal of Psychology*.

10 (*a*). Transfer from Sectional Membership to Sectional Associateship may take place on January 1st only, and after not less than six months' notice from the Member of his wish to transfer.

11. The procedure as regards the election of Sectional Associates shall be the same as that in the case of Members.

12. Every Candidate for election to the Society shall be recommended by at least two Members who shall be prepared to furnish information as to the Candidate's qualifications for membership. No Candidate shall be submitted for election to the Society who has not first been approved by the Council or by a Sectional Committee.

13. A Candidate who has been approved by the Council or by a Sectional Committee shall be nominated for election at the next General Meeting of the Society, or at the next meeting of the Section respectively.

14. One full week before the meeting at which the ballot is to take place, the Secretary shall send to each Member a balloting paper containing the names and addresses of the Candidates and the names of their proposers.

15. Any Member unable to attend a meeting at which a ballot is to take place can vote by sending his balloting paper to the Secretary.

16. One adverse vote in five shall exclude.

17. To vote *for* a Candidate, a Member must prefix a cross to the Candidate's name; to vote *against* a Candidate, he must erase the Candidate's name. Members who wish to record a vote neither for nor against a Candidate must leave the name untouched.

18. Any Member whose subscription remains unpaid for two years may, after receiving due notice, be removed by a resolution of the Council from membership of the Society.

19. Whenever it shall be proposed to remove a Member from the Society for any reason other than failure in payment of subscription, the matter shall be brought as a formal resolution before the Council or one of the Sectional Committees (whichever it shall most concern). In the event of the resolution being passed, a ballot shall be held for the removal of the said Member at the next General Meeting of the Society, or at the next meeting of the Section respectively. Should two-thirds of the Members voting vote in favour of the removal of the Member, that Member shall be removed from the Society.

20. If at any time it is desired to abolish any Section or Sections, to subdivide any Section or Sections, or to amalgamate two or more Sections, a Special Meeting of the Section or Sections concerned may be called for this purpose according to the conditions laid down in Rule 44. Resolutions passed at any such Meeting shall be reported by the Sectional Secretary or Secretaries concerned to the next Meeting of the Council. Should the Council approve of such resolutions, such resolutions shall

immediately take effect. But should the Council formally express disapproval of any or all of the said resolutions, these resolutions shall be considered by an Extraordinary General Meeting of the Society summoned in accordance with the conditions laid down in Rule 44 and the decision of this Meeting shall be final.

In the case of the desire to abolish any Section or Sections, the following alternative procedure may be followed. An Extraordinary General Meeting of the Society shall be summoned in accordance with the conditions laid down in Rule 44, and any resolution passed at this Meeting regarding the abolition of the said Section or Sections shall be final.

## OFFICERS, COUNCIL AND SECTIONAL COMMITTEES.

21. The business of the Society as a whole shall be conducted by a Council consisting of the President, Vice-Presidents, Treasurer, General Secretary, Librarian, Editors of *The British Journal of Psychology*, Chairman and Secretary of each of the Sections and of six Ordinary Members.

22. The President of the Society shall hold office for three years and shall not be eligible for re-election until after the lapse of a further period of six years. Every President shall at the termination of his period of office become a Vice-President for a period of six years. The other officers of the Society shall retire annually but shall be eligible for re-election.

23. Every year two ordinary members of the Council, having served for three years, shall retire and shall be ineligible for re-election until after the lapse of one year.

24. Names of Members to serve on the Council may be suggested by the Council or by any Member by sending the proposed names to the secretary at least two weeks before the Annual General Meeting.

25. Nominations for the election of Officers shall be handed in to the General Secretary before the Meeting preceding the Annual General Meeting and be announced at the Meeting preceding the Annual General Meeting.

26. One full week before the Annual General Meeting, the General Secretary shall send to each Member a balloting list containing: (1) The names of the Council; (2) The number of the attendance of members of the Council at the Council Meetings held during the year; (3) The names of those proposed by the Council or by Ordinary Members to fill the vacancies.

27. The Society may fill all the vacancies among the officers that may occur between two Annual General Meetings, by ballot in accordance with Rule 15 at the next General Meeting, due notice having been given. Similar vacancies among the ordinary members of the Council shall be filled by co-option by the Council.

28. In the absence of an ex-officio Chairman (in accordance with the conditions laid down in Rule 42) the Council shall elect one of its members to act as Chairman of Council. A quorum at a Council Meeting shall consist of five. If there be no quorum, the members attending shall have power to transact business, subject to the approval of absent members, to whom the resolutions adopted shall be forwarded.

29. The first meeting of each Special Section shall be convened by the General Secretary of the Society. At this meeting the Section shall appoint its Chairman, Secretary and Sectional Committee.

30. The Chairman and Secretary of a Section shall retire annually but shall be eligible for re-election.

31. The size of a Sectional Committee shall be determined by the Section. A quorum at a meeting of a Sectional Committee shall be as that Committee shall decide.

32. When Sections have been formed, Rules 12, 13, 14, 15, 23, 24, 25, 26, 27, 28, 40, 41, 42, 43, 44 shall (*mutatis mutandis* and unless expressly stated to the contrary) apply respectively to the Committees, Chairmen and Secretaries appointed by these Sections.

33. The Society shall not make any dividend gift, division or bonus in money unto or between any of its Members.

## SUBSCRIPTIONS AND FINANCE.

34. Each Ordinary Member shall pay an annual subscription of one guinea, which shall entitle him to receive a copy of *The British Journal of Psychology*, with the exception of such additional portions of the *Journal* as are devoted to special branches or aspects of Psychology and are published separately.

34 (*a*). Ordinary Members who are members of all Sections shall pay an inclusive annual subscription of £2. 12*s*.

35. In addition to the Ordinary Membership Subscription Members shall pay an additional fee for every Section which they join. This additional fee shall entitle members of any Section to receive such additional, and separately published portions of *The British Journal of Psychology* as may be devoted to the special branch or aspect of Psy-

chology studied by the Section. The amount of such additional fee shall be fixed by the Committee of the Section concerned in consultation with the Treasurer, and any resolution passed by the Sectional Committee with reference to such fee shall, before coming into force, receive the approval of the Council.

36. Sectional Associates shall pay a subscription the amount of which shall be determined by the Sectional Committee in consultation with the Treasurer of the Society.

37. A Member may compound for his annual subscription by a fee, the amount of which shall be determined by the Council after consultation with the Sectional Committee and the Treasurer. Such composition shall entitle the Member to all the privileges of membership, which he enjoys at the time of composition, for life.

38. The subscription is payable in advance and becomes due on January 1st. *The British Journal of Psychology* shall be sent only to Members whose subscriptions have been duly paid.

39. The Council shall grant to each Section permission to incur such expenses as may be necessary for the work of the Section; the approximate amount of such expenses to be determined by the Council.

## MEETINGS.

40. The Annual General Meeting shall be held in December, when the General Secretary's Report and the Treasurer's Statement of Accounts shall be laid before the Society.

41. The General Meetings of the Society shall be at such times and places as the Council shall decide, but in each year there shall be at least four meetings.

42. The President, or, failing him, a Vice-President, shall whenever possible, act as Chairman at the Meetings of the Council and of the Society. In the absence of the President or a Vice-President, some other Member shall, on the motion of the General Secretary or his substitute, be elected to act as Chairman.

43. The Secretary shall send to all Members of the Society notices of each meeting at least seven days beforehand, and of the business to be transacted at the meeting.

44. An Extraordinary General Meeting may be summoned at any time by the Council, or by the General Secretary on the written request of twenty Members; at least fourteen days' notice of such meeting shall be sent to each Member of the Society.

## VISITORS.

45. Each Member shall have the privilege of introducing two visitors at any meeting. The Council or a Sectional Committee shall however have the power of closing any meeting or meetings to visitors, provided notice to this effect be given on the notice convening such meeting or meetings. The names of visitors together with the names of Members introducing them shall be entered in a book provided for that purpose. Any Member of the Society attending a meeting of a Section to which he does not belong must be introduced by a member of that Section.

## BRANCHES.

46. The Council shall be empowered to sanction and dissolve the establishment of Branches of the Society throughout the British Empire. Members of such Branches shall pay the same subscription as that of Ordinary Members direct to the Treasurer of the Society and be entitled to the same rights and subject to the same rules as Ordinary Members. The Council shall be empowered to refund to the Treasurer of any Branch such proportion of Branch Members' subscriptions as the Council may determine to meet local expenses.

46 *a*. Any Rules made by Branches of the Society shall be first submitted to the Council for their approval.

46 *b*. The minimum number of Members necessary to form a Branch shall be 10.

46 *c*. Branches shall be required to present a report to each Annual General Meeting.

## RULES.

47. The Rules of the Society may be altered at the Annual General Meeting, after notice of alteration has been given at a previous Ordinary General Meeting, or they may be altered at an Extraordinary General Meeting summoned for that purpose. Such Extraordinary General Meeting shall be held in London, and the purpose for which it is called shall be expressly stated in the notice convening the Meeting. No other business shall be transacted thereat.

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CAMBRIDGE: PRINTED BY W. LEWIS, M.A., AT THE UNIVERSITY PRESS